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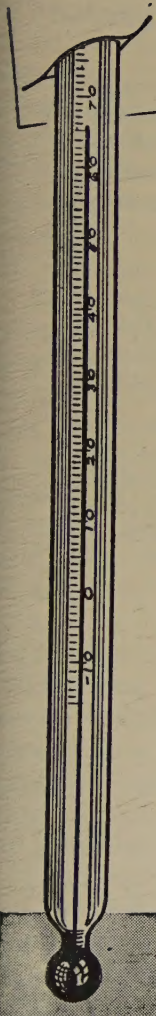

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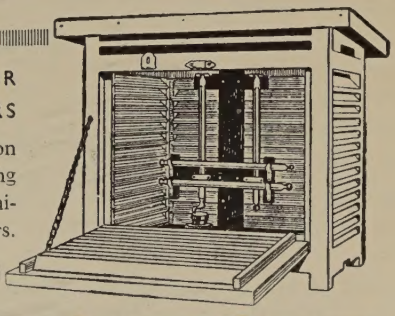

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EDITORIAL

The request that I should write an Editorial for the *Quarterly Journal* was one which I accepted gladly, as it affords the opportunity to say something of the meteorology of the last 37 years, the years during which I have been concerned with it.

My initiation into meteorology occurred in the earliest days of 1916, when I entered the Meteorological Office as a Junior Professional Assistant. My first studies of the subject consisted in reading Shaw and Lempfert's *Life history of surface air currents*, a better introduction than many that might have been suggested. It gave the reader who had no initial prejudices a picture of a mechanism of development which no other publication then available could have given. After about five months in the Meteorological Office at South Kensington I was posted to the Meteorological Section, R.E., at G.H.Q. in France, and spent the remainder of the war occupied with problems of the Army or of the R.F.C.

We learned to use upper-air data in a restricted sense during the war of 1914-18 and by the end of the war meteorologists were looking forward to a very rapid improvement in weather forecasts, as the result of the interchange of upper-air data over a wide area. I would say unhesitatingly that this hope was not realized in the inter-war years, any improvement in forecasting which may have occurred being in larger measure due to improvements in communication, by which observations became more quickly available, and to increased frequency of observations. Even when we compare the efficiency of forecasting today with that of, say, thirty years ago, we must first make due allowance for improvement in communications, increased frequency of observations, and new methods of observation, such as radar wind determinations, before we begin to assess the improvement in the work of the forecaster himself. If we carry out a comparison in this way, what we shall find is a great improvement in the methods of representing upper-air data, in such a manner as to give a reasonable picture of the three-dimensional nature of the atmosphere, with a consequent improvement in the appreciation of the development of weather. The extent of the improvement in forecasting, regarded as a scientific exercise, during the last thirty years, is still an unanswered question, and I am not prepared to go further than to say there has been *some* improvement.

Much might be written of the effects of meteorology on warfare, as well as of the effects of war on meteorology. Each of the two World Wars, by bringing new meteorological problems into prominence, stimulated the developments of meteorology, while the realization of the importance of these new problems moved the fighting services to assist meteorology by providing money and facilities for which we might otherwise have waited a century. This is of course true of almost all the sciences, but meteorology appears to have shown an especial tendency to blossom in human disaster, like the flower on a bombed site. When the Tay Bridge was destroyed by an

exceptionally strong gust in a gale on 28 December 1879, it became necessary make an investigation of the strength of winds in gusts. From this investigation can the invention of the Dines Pressure-Tube Anemometer, and the principle enunciat by Dines that all anemometers should have open and comparable exposures, if the records were to be capable of comparison. Again, when the S.S. *Titanic* was sunk 1912, as the result of a collision with an iceberg, this led to the setting-up of investigation into the ice conditions in the North Atlantic. The meteorological pa of the investigation was carried out by G. I. Taylor, the immediate results of h investigations being incorporated in papers which laid down two separate lines attack on the problem of turbulence, and which brought out very clearly the preci mode of formation of the fogs which frequently formed over the icy regions of th North Atlantic.

During the last eighteen years I have been concerned with the teaching meteorology to post-graduate students of the University of London, at the Imperi College of Science and Technology, South Kensington. The Department Meteorology at Imperial College, over which I shall have ceased to preside by the tim these lines appear in print, has, like the weather, been in a continual state of develo ment, but unlike the weather its ' tendency ' has been almost continuously positiv Thanks to the sympathetic view taken both by Imperial College and by the Universi of London, funds have been made available for the equipment of workshops an laboratories, and for an expansion of the staff of the department from an initial val of one in 1934, to a present number of 16 (September 1952). As a result, there h been a considerable expansion of the fields of study and research pursued, addi greatly to the interest of us all.

My connection with Imperial College actually goes back to the period 1920-2 during which Sir Napier Shaw held the part-time Professorship of Meteorology whic had been instituted by Sir Richard Glazebrook within the Department of Aeronautic At Sir Napier's request I gave a course of about 32 lectures in each of the four sessio of his tenure of the Professorship. The lectures were attended by a number of membe of the staff of the Meteorological Office, as well as by oversea meteorologists w came to visit England, and spent some time with Sir Napier. It is impossible to loo back on that time without wondering how much my students gained from tho lectures. But the discipline involved in having to produce a reasonably logical cour of lectures, in the absence of any textbook which covered more than a small part the syllabus, was extremely valuable. I was forced to read widely in the literature meteorology, and I now realize that the lecturer learned much from those lecture whatever the students may or may not have learned. Besides, I liked lecturing, an usually after each lecture I had a long talk with Sir Napier, the talk sometimes leadi to a lengthy argument, from which one could not fail to gather information, and oft inspiration. During a long period of years I enjoyed a close friendship with Sir Napi and had many discussions with him on a wide variety of meteorological problem

The period of my own tenure of the Professorship of Meteorology at Imperi College, which only became a full-time Professorship when I was appointed, h seen a certain advance in the status of meteorology within the University. In t early years there was a distinct tendency to regard meteorology as an easy optio but this attitude has now changed. We have always regarded meteorology as on suitable for post-graduate study, as it requires a sound knowledge of physics a mathematics, and moreover demands a maturity of judgment which is not to be look

for at the undergraduate stage. An honours degree in physics or mathematics has always been required as an essential qualification in entrants, and this requirement has only been eased in a few special cases. My own advice to would-be meteorologists is that they should first take a B.Sc. degree with honours preferably in physics, with some mathematics in addition. Meteorology is the physics of the atmosphere, and what meteorology of today requires above all else is an injection of new physical ideas. Mathematics is rather like a coffee-grinding machine, which presents in a new form what is initially put into it. In saying this I do not wish to minimize the value of some of the dynamical studies published during the last twenty years, though I should like some competent person to write a critical analysis of their value.

However interesting meteorology may be as an academic study, it has still wider implications. In my view meteorology should be regarded as one aspect of a wide human culture, having points of contact with almost every variety of human activity. Reference was made earlier to the importance of meteorology in certain forms of warfare, during the two World Wars. Many peace-time human activities are equally affected by meteorological factors. For example, soaring flight might be defined as applied meteorology, and is indeed recognized as a method of obtaining meteorological information which is only to be got with difficulty by any other method. Since 1935 the Department of Meteorology at Imperial College has always had at least one member of its staff who took an active interest in the activities of the British Gliding Association. Other examples of the close connection that should exist between meteorology and other interests can readily be cited, as for example the need for careful consideration of the meteorological conditions of suggested sites for industries of a special type. Both the siting and design of an Electricity Generating Station should take account of such conditions.

Far too many men build houses and plant orchards or gardens without giving any consideration to the meteorology of the site they choose ; and far too often is the meteorologist asked to help to protect orchards from frost damage, instead of being asked to advise on the selection of site for the orchards. And perhaps a day will come when the meteorologist, along with the physiologist and the economist, will be invited to help to delimit those regions of the earth's surface within which the white man should not attempt to settle permanently.

Finally, let me add that, like so many other workers in the field of meteorology, I have frequently felt impatient at the slow rate of progress in meteorology, and even wondered whether our science would in fact ever advance. It is clear that the founders of the Meteorological Society believed that if sufficient observations were made and plotted, the mysteries of the atmosphere would become manifest. The extent of their disillusion may be gathered from Sir Napier Shaw's description of the state of things in the Meteorological Office when in 1897 he first became a member of the Meteorological Council. Again, at the end of the 1914-18 War, we believed that a very rapid advance in meteorology, leading to a rapid improvement in the accuracy of forecasting, would come from the exchange of upper-air data over a wide area, and this hope too was doomed to disappointment. The hopes of today are based on more solid foundations, and I am bold enough to believe that our subject is now beginning to make real progress. Moreover, I now believe that, instead of bewailing the slowness of our rate of progress, we should marvel that progress should ever have been achieved in so complicated a subject as ours.

DAVID BRUNT

551.511 : 551.55

Turbulence and transfer processes in the atmosphere

By C. H. B. PRIESTLEY and P. A. SHEPPARD

Section of Meteorological Physics, C.S.I.R.O., Melbourne, Australia; Imperial College, London

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VII. Epilogue

I. Introduction and scope

1. Delegates to a recent international conference on 'Atmospheric turbulence in the boundary layer' (many of whose papers are herein referred to) spent some time in discussing possible definitions of turbulence without achieving any real progress towards a generally acceptable formulation. But while the abstract concept has at present no satisfactory definition, there is no doubt about the purpose of the study of turbulence, and of atmospheric turbulence in particular, in the wider subject of

meteorology. The aims are the identification and quantitative understanding of the effects wrought on atmospheric phenomena at one scale by the (extra-molecular) motions occurring on smaller scales. The latter can commonly not be specified individually so that the study must then devolve entirely on statistical techniques. These techniques, as applied to atmospheric motions on whatever scale, determine the scope of the present review.

The familiar equations of Navier-Stokes are an expression of Newton's second law of motion for a continuous medium for which velocity, density and stress (normal and tangential) at a point may be defined in terms of somewhat idealized instrumental procedures. These equations and those of continuity, state and heat transfer govern the totality of possible fluid motions and if our observations could be made in conformity with the definitions then all problems of fluid flow would essentially be problems in laminar flow and, in principle, soluble in full detail. We are however rarely able and indeed have no general desire to observe such details but are rather interested in a particular scale of flow, from the general circulation downwards to boundary-layer flow. So we enquire how to manipulate the equations of motion, heat transfer, etc., so as to suppress the ' embroidery ' or rather so as to express its effects in terms of more readily measured quantities, noting that the coarser embroidery of one scale is itself a main flow of smaller scale.

From the smallest disturbance in the earth's boundary layer up to the cyclones, anticyclones and long-wave patterns, which are the disturbances of the general circulation, the spectrum of atmospheric-eddy sizes appears to be unbroken, and we shall attempt a review of current knowledge over the whole range. In view of the above-defined scope the details of the eddying motion will not be discussed, but it is worthy of note that these details are, in fact, unknown at any point but the upper end of the scale and even there both factual knowledge and theoretical explanation contain many gaps. Since the review must reflect the development of the subject it will be much concerned with the smaller scales (notably boundary-layer problems), a little with the grandest scale, but hardly at all with the large intermediate range of eddy motions which have been so little studied. We shall, however, pause here and there to point the need and the possibility, for it is our belief that the turbulence technique is potentially capable of advancing many of the intermediate-scale problems yet untackled. The continuous nature of the spectrum does itself promote this belief, and suggests that the search for similarities over, at least, large ranges of the spectrum would be profitable. Equally of course one must be aware of the differences, essentially of energy supply and of the ability of the Coriolis force to exercise control on a motion whose ' period ' extends beyond a day or so, so that some differences in the ' laws ' of turbulence, according to scale and energy supply, are to be expected.

At this point we may note that some of the complications already referred to in atmospheric turbulence disappear completely when turbulence in the laboratory is considered. Whereas there is no upper limit to the size of atmospheric eddies – though bounded in space by the size of the earth, they extend in time to the scale of climatic fluctuations – so many problems of the tube and tunnel are concerned with relations between the turbulence and *the* mean flow. Another, and probably more significant, simplification is that in the tunnel the energy input is on the scale of the mean motion, whereas in the atmosphere it is distributed through a substantial range of the intermediate scales and apportioned therein in a manner of which, quantitatively speaking, we are often ignorant. We must also add the atmospheric complications associated

with thermal stratification and with an inhomogeneous horizontal distribution of temperature and other basic variables. Wind-tunnel turbulence is, in consequence, the more easily studied and more advanced of the two and any present account of atmospheric turbulence must lean heavily on the findings in this field. But an over-leaning in this respect and the search for parallels in the two fields where none may really exist has tended in the past to thwart the development of the atmospheric side in its own right.

Our aim here is to present, as far as is practicable, an account of the (statistical) properties of atmospheric turbulence, of the factors controlling its nature and intensity, and of its effects. It is of course on the effects, in the form of transfers of momentum, mass and energy, that the practical importance of the subject rests. For long it has been thought that these transfers are equal to the mean gradient of the corresponding quantity multiplied by an index of activity (the transfer coefficient). This view, though now appearing over-simplified, reflects the important principle that turbulence and transfer are governed by the macroscopic condition; while the latter, in turn, is to a greater or less extent determined by the turbulence, for this is the very essence of our problem as defined in the opening paragraph. Such an intimate inter-relation suggests that an account of the facts about the macroscopic condition, or mean profiles when we are considering transfers predominantly in one direction, should form an important part of our present task. In the sections on turbulence in the boundary layer we have followed tradition in this respect. But we wish to be excused from the logical parallel of describing the complete vertical structure of the atmosphere, as the background for convection and middle and large-scale turbulence, and, for the last-named again, the mean horizontal profiles on the global scale. These facts are given in other places while the knowledge of turbulence on these scales is not yet sufficient to make the juxtaposition worth while.

Apart from the broadening of scope to include the larger-scale turbulence this review differs from others in the subject in giving some account of modern ' statistical ' theories of turbulence. The importance of these theories, concerned largely with the smaller-scale components in which the turbulence is isotropic, to meteorology, has yet to be determined and may immediately be restricted to certain aspects of diffusion. An understanding of the structure of the smaller motions forms however a necessary background to our knowledge while the theories, if only applicable to the smallest atmospheric motions, would lead to a determination of the isotropic threshold and so provide the instrumental meteorologist with a measure of the position in the scale to which, but not below which, the resolving power of his instruments for transfer measurements should extend.

II. The onset, stability and characteristics of turbulence

2. *Transition from laminar to turbulent flow; associated effects*

Reynolds, as is well known, found that the transition from laminar to turbulent motion depended on a critical value of a non-dimensional parameter $R\,(=\bar{u}\,d/\nu)$ being reached. Here $\bar{u}$ is the mean velocity of flow of fluid with kinematic viscosity ν through a tube of diameter d. The parameter R, now called the Reynolds number, expresses the ratio of the local acceleration due to fluid inertia, i.e., of terms like $u\,\partial u/\partial x$ in the usual notation, to that due to molecular viscosity, i.e., of terms like $\nu\,\partial^2 u/\partial z^2$. The transition was demonstrated by the enhanced diffusing power of the

fluid, using a filament of dye, but equally important was the enhanced transfer of momentum to the containing walls as shown by the greater pressure difference required to maintain a given mean flow through the tube. Thus turbulence increased the effective viscosity. But the form of the law of resistance also changed, from linear dependence on $\bar{u}$ to $\bar{u}^{1.73}$, *so that turbulence could evidently not be regarded as merely increasing the viscosity uniformly through the fluid.*

Transition from laminar to turbulent flow over a flat plate is similarly determined by the Reynolds number. The change takes place at a distance x downwind of the leading edge such that $U x/\nu \simeq 300{,}000$ (Dryden 1934) where U is the 'free-stream' velocity. If, however, x is replaced by the thickness δ of the *boundary layer*, that is the layer of fluid in which the velocity is less than the free-stream value ($\delta = 1{\cdot}7\,(\nu x/U)^{1/2}$, cf., Goldstein 1938), one obtains a critical Reynolds number $\simeq 1{,}000$, which is about the same as for transition in a tube with radius now the appropriate characteristic length. Following transition a thicker, turbulent boundary layer builds up and again the law of resistance changes, from $U^{1.5}$ to $U^{1.8}$ dependence.

There is no obvious quantitative application of these results to the atmosphere, even in neutral stability, although vague analogies have been drawn from time to time and there is perhaps something in common between the boundary layer over the flat plate and the terrestrial boundary layer. We shall however find a use for the Reynolds number in atmospheric as in other turbulence in Chapter VI.

3. *Specification of turbulence*

Given a field of flow in which the velocity components and other fluid properties such as pressure, temperature, etc., are highly variable from point to point and from instant to instant, Reynolds (1895) showed how the field might be regarded as made up of mean and turbulent 'components' and his analysis is particularly instructive for the meteorologist. Consider for example a single velocity component u at a fixed point; a mean value $\bar{u}$ is formed and identified with a given instant of time t by meaning u over an interval of time τ centred on t, i.e., $\bar{u} = \frac{1}{\tau}\int_{t-\tau/2}^{t+\tau/2} u\,dt$. As τ is increased from some value comparable with the interval between the more rapid fluctuations of u so $\bar{u}$ will itself show appreciable fluctuation with τ, but this fluctuation will in many cases diminish to a relatively small amount when τ is sufficiently large to contain a very large number of the fluctuations. *Only then indeed is it unambiguous to speak of the mean motion.*

The concept of spatial or spatio-temporal mean values may be similarly approached and while there may be no significant difference between the differently formed means in many cases, there may be some fields of flow in which one type of mean is more appropriate than another. When q is any independent variable (x, y, z, t) and a, b any pair of fluctuating dependent variables, then

$$\overline{\frac{\partial a}{\partial q}} = \frac{\partial \bar{a}}{\partial q} \qquad . \quad . \quad . \quad . \quad . \quad (3.1)$$

It is usual to assume also that

$$\overline{ab} = \bar{a}\bar{b} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (3.2)$$

The conditions governing the truth of this postulate have yet to be resolved; that both a and b should vary linearly appears to be neither necessary nor sufficient.

Having arrived at a specification of the mean, Reynolds proceeds to the specificatio of the turbulent or eddy component u' at an instant t as the departure of the actu flow from the mean for that instant, i.e., $u' = u - \bar{u}$. (We retain temporal mean unless otherwise stated.) Subject now to the important provision, especially fo meteorological purposes, that $\partial^2\bar{u}/\partial t^2 = 0$ we find

$$\overline{u'} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.$$

and similarly for other fluctuating quantities, the bar implying a mean, over the sam interval τ as before, of the instantaneous departures u' from the *u for the same instant*. If $\partial^2\bar{u}/\partial t^2 \neq 0$ the difference of $\overline{u'}$ from zero is of order $\frac{1}{2}\frac{\partial^2 u}{\partial t^2}\tau^2$.

In the atmosphere, scales of motion are not sharply differentiated and it follows that a τ cannot be found which is long compared with the period of every 'eddy' comprised within it. We shall have cause to refer more than once in this review to the implications of the non-realization of Eqs. (3.2) and (3.3).

4. *Effect of turbulence on the mean motion*

Mean motion and turbulent motion are now specified in principle, and we may immediately proceed to the central problem, namely in what manner are the properties of mean motion determined by the presence of a superposed turbulent field? The practical significance of this problem arises not least from the fact that many instruments used in the routine measurement of the physical properties of a fluid in motion are incapable of following the rapid turbulent fluctuations, while, if they were so capable, the amount of information thus made available would be so great as to be a positive embarrassment to the study of the broader features of the flow. (In this context the synoptic chart familiar to all meteorologists is in essence an 'instrument' which suppresses spatial variations by an amount depending on its scale, the instrumental and other values on which it is based having already suppressed the smaller-scale variations). Reynolds (1895) provided a formal answer to the problem by taking mean values of each term in the Navier-Stokes equations of motion. The result is well known; that the mean motion is determined not only by the external (gravity), pressure and viscous forces but by additional stresses, normal and tangential, deriving from the turbulent velocity fluctuations. If u', v', w' are turbulent components of velocity in rectangular co-ordinate directions x, y, z and ρ is the density at a point, then the mean normal hydrostatic pressure p is augmented by $\overline{\rho u'^2}$, $\overline{\rho v'^2}$, $\overline{\rho w'^2}$ in the respective co-ordinate directions, and the additional shearing stresses in the x, y planes are $-\overline{\rho u'w'}$, $-\overline{\rho v'w'}$ in the x, y directions respectively, with corresponding shearing stresses in the other two planes. Of these *Reynolds stresses*, (which are fluxes of momentum with signs reversed) the normal stresses are necessarily finite, but non-zero values of the shearing stresses depend on the existence of correlation between the component eddy velocities in the respective products.

In the full expansion of the Navier-Stokes equations terms in $\overline{\rho uu'}$, $\overline{\rho uv'}$, etc. also appear. Reynolds equated these to zero by invoking, in order, lemmas (3.2) and (3.3) but it appears that in the atmospheric case this simplification is not justifiable, as has been pointed out in another context by Swinbank (1952). More will be said on this in Section 9 where the formulation of the transfer of momentum, with that of other properties is approached from a somewhat different angle. The inverse problem, that

f determining definitions of the mean (alternative to the simple running mean f Reynolds) which allow the Reynolds equations to be *rigorously* derived, is the ubject of a recent note by Kampé de Fériet (1951).

Meanwhile we note that, if we could solve the full equations for the instantaneous notion in given simple fields of flow we should be able to evaluate the Reynolds stresses nd indeed infer all the properties of the complete motion. This has not been done nd it may be doubted whether hydrodynamics, as at present formulated, is even uited to such an essay. The difficulty resides not merely in the non-linearity of the quations but in reducing the complexity to manageable proportion.

More tentative methods of attack have therefore been used; on the one hand o use simplified forms of the equations of motion (with their corresponding restrictions), r to approach the problem from the equations of energy; on the other to make easonable *ad hoc* assumptions about the nature of turbulence and infer such quantities s the Reynolds stresses in terms of the mean motion itself and one or more constants o be determined empirically. We shall deal with the former in this chapter and vith the latter in Chapter III.

Before considering such theoretical procedures we may note, from experience, that he eddy* pressures (or their gradients) are quite negligible compared with the tatic pressure (or its gradient), and are therefore incapable of affecting the *mean* notion – the properties of the turbulence are another matter. In regions of shear owever, the eddy tangential stresses generally far outweigh the true viscous stresses, .g., $-\overline{\rho u'w'} \gg \bar{\rho}\,\nu\,\partial\bar{u}/\partial z$. The existence of a correlation between u' and w' in a egion of shear in the direction of z is obvious enough in a qualitative sense, if we eglect pressure forces, motions in the z-direction tending to provide a product $u'w'$ f sign opposite to that of $\partial\bar{u}/\partial z$. Where no such correlation exists there is no mmediate control by turbulence of the mean motion, but the diffusive properties of he field for other fluid properties than momentum will of course remain. This aspect vill be more fully considered in Chapter V.

5. *Studies of the stability of laminar flow*

Basic theoretical studies of the generation and maintenance of turbulence have ollowed two main lines. From Reynolds (1895) onward, one line has been to letermine the conditions under which some *small* initial disturbance in a prescribed aminar flow, e.g., pressure flow between parallel planes, would increase – the method f perturbation mechanics. The literature is large and the subject complicated and difficult to summarize, (see Prandtl 1935, Goldstein 1938, Holstein 1950), but the heory can be, and indeed has been, a useful guide in experimental work.

Thus Tollmien (1929, 1935) and Schlichting (1933, 1935a) computed the Reynolds umber at which two-dimensional disturbances imposed on a laminar (Blasius) boundary ayer over a flat plate, without pressure gradient, would grow. The critical Reynolds umber was found to depend on a quantity $n\nu/U^2$ where n is the frequency of the periodic) disturbance and U the free-stream velocity, in the manner shown by the ull line of Fig. 1. Thus, a two-dimensional disturbance moving along the boundary ayer will be damped or amplified according as the frequency and the Reynolds number, he latter increasing downstream with increasing thickness of the boundary layer, give

* We use the word 'eddy' synonymously with 'turbulence' or 'turbulent' – it does not imply the existence of discrete asses of fluid enjoying a more or less independent life (*verb. sap.*).

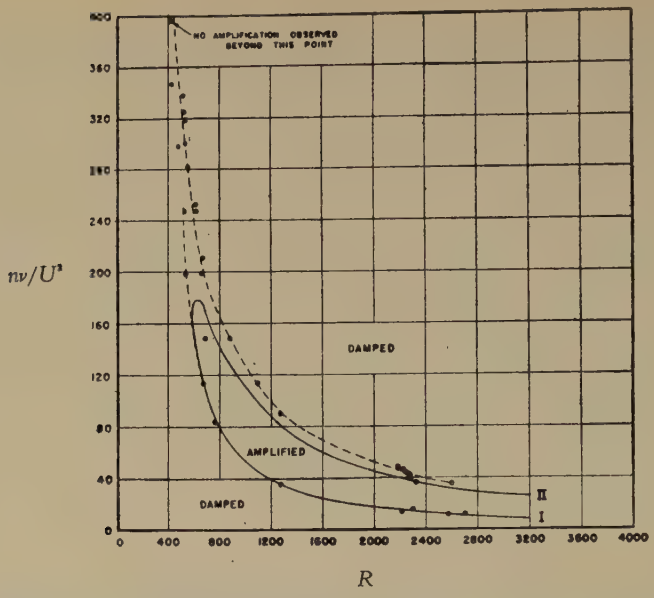


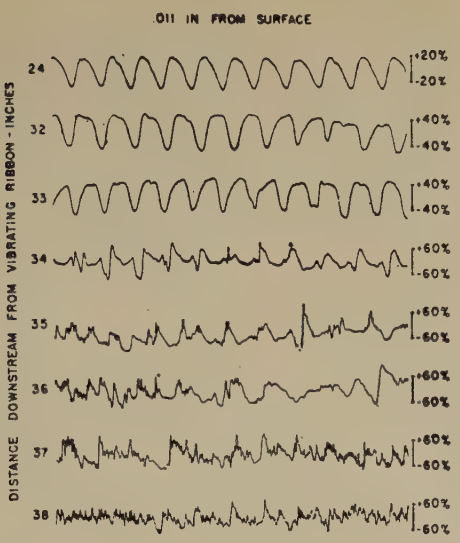
Figure 1. Instability in a laminar boundary layer over a flat plate. The full line (I, II) bounds a regi of amplified two-dimensional oscillations from an outer region in which such disturbances are damp (after Schlichting 1935 a). The points and pecked lines show these regions as observed by Schubau and Skramstad (1947) for disturbances produced by a vibrating ribbon in the boundary layer (Reproduc by kind permission of the National Bureau of Standards, U.S. Department of Commerce).

a point lying without or within the loop. The theory has received very beautif experimental verification (cf., Fig. 1) by Schubauer and Skramstad (1947). Schlichtin also computed contours of constant amplification within the loop, and Schubauer ar Skramstad verified their general form though the measured values were rath smaller.

The Tollmein-Schlichting theory is not a theory of turbulence but only boundary-layer instability. However given unstable oscillations, it is not difficult imagine the further process of transition to full turbulence, and Schubauer ar Skramstad observed such transition from the amplified oscillation (cf., Fig. 2). As result of the increase in amplitude of the oscillations, the velocity profile probab develops points of inflexion where unstable back-flow occurs, vortices then form ar spread out rapidly. But ignorance of the theoretical behaviour of large-amplitu waves implies lack of understanding of just where transition will occur. Both theo and experiment have however shown that the frequency distribution of the initi disturbance (regarded as a Fourier synthesis) is of great importance since amplificatio only occurs in a narrow band of frequencies from among those present, providin transition. The disturbance may arise within the boundary layer itself, from surfa irregularities, or may be impressed by the free stream.

Schlichting (1935b) extended his analysis of boundary-layer instability to the ca of a boundary layer with thermal stratification $\partial\overline{T}/\partial z$. He found that instability shou set in for a value of the non-dimensional ratio

$$\frac{\frac{g}{\overline{T}}\left(\frac{\partial\overline{T}}{\partial z}+\Gamma\right)}{(\partial\overline{u}/\partial z)^2},$$



gure 2. Oscillograms of longitudinal velocity fluctuations at increasing distances downstream from a rating ribbon placed in the laminar boundary layer of a flat plate. (Frequency of vibrations 70/sec, = 50 ft/sec). Note first the growth in amplitude of the periodic disturbance as shown by the change scale on the right, and the later transition to fully developed turbulence (after Schubauer and Skramstad. produced by kind permission of the National Bureau of Standards, U.S. Department of Commerce).

w called the Richardson number Ri (for reasons which will appear), of between 0409 and 0·029, the value increasing with increase of a further non-dimensional rameter, the Froude number $U^2/\delta g$, where δ is the thickness of the boundary layer, is gravity and Γ the adiabatic lapse rate. Reichardt (1938) is quoted by Schlichting having found experimental evidence for his criterion.

The possible application to the atmosphere, which is always turbulent in some gree, of these boundary-layer perturbation theories is limited to the smaller scales. espite the continuity of the atmospheric spectrum one must at the present time go most to the other extreme, i.e., to the synoptic scale, before encountering any compar-le development in the theory of unstable perturbations. This body of work is too ge for complete reference here, but we may note that Eady (1949) finds the instability large-scale perturbations to be determined by the large-scale value of Ri, in which e shear arises from the horizontal temperature field (thermal wind) and not directly om the presence of a boundary.

The extension of perturbation theory to provide a theory of fully-developed eddy otion is of course the major requirement in the theory of turbulence, but of its alization, for aerodynamicist or meteorologist, there is as yet no sign.

6. *Stability of turbulent flow; energy considerations*

The work described in Section 5 is not to be confused with studies which seek teria for the growth or decay of turbulence which is already in existence, an important se generally and not least in the atmosphere. In this problem Reynolds (1895) was ain the pioneer. Framing the equations of energy for a homogeneous fluid from e equations of motion he showed that, within a body of fluid sufficiently large to ake surface actions negligible, the energy of turbulent motion will grow or decay

according as the supply from the mean motion through the Reynolds stresses exce or falls short of the viscous dissipation. The source is expressed in terms such $-\bar{\rho}\,\overline{u'w'}\,\partial\bar{u}/\partial z$ and that it will be generally positive follows from the earlier conclus that $-\bar{\rho}\,\overline{u'w'}$ is likely to be of the same sign as $\partial\bar{u}/\partial z$ (momentum flowing *down* gradient). But there may well be an upper limit of scale for which this inferenc valid, and the whole treatment depends on the separability of the mean and the turbu flow. The dissipation D, a necessarily positive quantity, is represented in terms s as $\mu\overline{\left(\frac{\partial u'}{\partial x}\right)^2}$ and $\mu\overline{\left(\frac{\partial u'}{\partial z}+\frac{\partial w'}{\partial x}\right)^2}$, where μ is dynamic viscosity. Since the rate production and decay both depend on products of the eddy velocities, the stabi of a given motion is independent of the intensity of the turbulence. Direct verificat of the quantitative importance of the viscous dissipation in flow between para planes has been obtained by Taylor (1935) and Laufer (see Section 7b).

The atmospheric case presents a more complex problem in that changes in inter and potential energy occur and work done against gravity by the eddy motion (w against buoyancy) and *also by the mean motion* must be allowed for. External h sources too are present, in fact we know these to be the ultimate source of all atmosph motions.

Approach may be made either through the full equations of motion, or m directly from energy conservation. Richardson (1920) chose the latter in consider the energy changes in an atmospheric block of clear air heated or cooled from bel Neglecting radiation and, for a large block, the rate at which the surroundings w on it the equation of energy conservation for the system is :

$$\frac{\partial E}{\partial t}=\frac{\partial \bar{E}}{\partial t}+\frac{\partial E'}{\partial t}=G-\frac{c_p}{c_v}\frac{\partial I}{\partial t} \quad . \quad . \quad . \quad (($$

where E, $\bar{E}$, E' are the kinetic energies of the total, mean and turbulent flows respectiv G is the rate at which heat energy is flowing vertically into the system, and $(c_p/c_v)\,I$ sum of the internal and potential energies (total heat). It has been assumed that th is no diffusion of eddying energy and no advective change. Compressional energy included in the total heat so that pressure does not appear explicitly though it concerned in the mechanism by which the heat flux G is effected.

Richardson showed that the heat G entering the system plus the viscous dissipat D exceeded the change in total heat by a quantity proportional to the integrated ec flux of heat throughout the volume V, or

$$G-\frac{c_p}{c_v}\frac{\partial I}{\partial t}=-\int_V \frac{g\bar{\rho}K_h}{\bar{T}}\left(\frac{\partial \bar{T}}{\partial z}+\Gamma\right)dV-D \quad . \quad . \quad ((($$

where K_h is the eddy conductivity (see Chapter III). This excess he assumed to h source of *turbulent* kinetic energy (' The eddies are themselves the engines by wh the available heat is converted into their visible motion '). The other source, follow Reynolds, was identified as $-\bar{\rho}\,\overline{u'w'}\,\frac{\partial\bar{u}}{\partial z}$ etc., which may be written (see Chapter as $\rho\,K_m\left(\frac{\partial u}{\partial z}\right)^2$ etc. where K_m is the eddy viscosity. He thus obtained*

$$\frac{\partial E'}{\partial t}=\int_V \bar{\rho}\,K_m\left[\left(\frac{\partial\bar{u}}{\partial z}\right)^2+\left(\frac{\partial\bar{v}}{\partial z}\right)^2\right]dV-\int_V\frac{g\bar{\rho}K_h}{\bar{T}}\left(\frac{\partial\bar{T}}{\partial z}+\Gamma\right)dV-D \quad ((($$

* Richardson in fact omitted D explicitly from Eq. (6.2) and re-introduced it in Eq. (6.3) with the wrong sign, which led to some confusion.

hich would provide a *measure* of the rate of increase of eddying energy when the efficients and the rate of dissipation are known or estimated. The dissipation term ill prevent indefinite growth of turbulence in superadiabatic conditions and increase ie shear required to maintain turbulence in stable conditions. We shall not here cept Richardson's further simplification that $K_m = K_h$. Finally he purported to ow by a rough analysis that the viscous dissipation would be negligible compared ith the other quantities and so presented a criterion that the kinetic energy of *eddying* otion would increase or decrease according as

$$\int_V K_m \left[\left(\frac{\partial \bar{u}}{\partial z} \right)^2 + \left(\frac{\partial \bar{v}}{\partial z} \right)^2 \right] dV \gtrless \int_V \frac{g K_h}{\bar{T}} \left(\frac{\partial \bar{T}}{\partial z} + \Gamma \right) dV \quad . \quad (6.4)$$

ealizing the uncertainty of his result regarding viscous dissipation, Richardson escribed his criterion as one of 'just-no-turbulence.' As such it would indeed be criterion of instability, appropriate to the onset rather than the growth of turbulence, ut the interpretation seems to us misleading for the analysis relates essentially to a isturbed field in which the mean values $\overline{u'w'}$, etc., are assumed to exist.

Swinbank (1952) has recently questioned Richardson's analysis on fundamental rounds. We have already expressed doubts as to the validity in the atmosphere of uch simplifications as $E = \bar{E} + E'$, though a way round the difficulty, suggested in ection 9, may also be applicable here. Leaving this aside, Swinbank's second objection that Eq. (6.2) has been assigned in Eq. (6.1) entirely to $\partial E'/\partial t$, leaving no part of is source available for maintaining the mean flow. Until physical justification has een supplied it must appear an arbitrary and dangerous procedure to assign the hole of the energy source to a single limited part of the spectrum (more will be said f this aspect in Chapter VI). Indeed the energy approach, which is not designed to eal with mechanisms, is in itself incapable of differentiating between the mean flow nd the turbulent energies and therefore can only lead to a criterion for the *total* inetic energy. This must, from Eqs. (6.1) and (6.2), and within Richardson's estrictive assumptions, increase or decrease according as

$$-\int_V \frac{g\bar{\rho} K_h}{\bar{T}} \left(\frac{\partial \bar{T}}{\partial z} + \Gamma \right) dV \gtrless D \quad . \quad . \quad . \quad (6.5)$$

hich is the form of the energy criterion advocated by Swinbank. It is a striking ontrast that the critical lapse rates in (6.4) and (6.5) are respectively on the (statically) table and unstable sides of the adiabatic.

The treatment so far has been concerned with a large block and the results cannot, wing to the nature of the assumptions made, be read as applying at a point though ichardson did, in dropping the integrals in (6.4), imply some support for such a eading. It is evident then that, in order to consider the growth or decay of turbulence t a point, we must return to the more detailed considerations provided by the equations f motion. Calder (1949a) – Van Mieghem (1949) has also given a detailed analysis – reats the case of turbulence of not too large a scale for which the relative density ariations are not therefore substantial and is thus enabled to write $\partial u'/\partial x + \partial v'/\partial y + w'/\partial z = 0$ from the equation of continuity. The density variations are retained lsewhere where they are thought to be significant.

Forming, then, the energy equations of mean flow and total flow and subtracting, alder obtains an expression for the rate of change of eddy energy per unit volume

following the mean motion in which Richardson's term $-\frac{g\bar{\rho}K_h}{\bar{T}}\left(\frac{\partial\bar{T}}{\partial z}+\Gamma\right.$ replaced by two terms: (i) $-g\overline{\rho'w'}$, the rate of working of the buoyancy for (ii) $-(\overline{u'\,\partial p'/\partial x}+\overline{v'\,\partial p'/\partial y}+\overline{w'\,\partial p'/\partial z})$, the rate of working of the fluctua pressure gradients. Two other terms do in fact appear, a rate of working of visc stresses on the eddy motion $\left(\frac{\partial}{\partial x}(\mu\,\overline{u'\,\partial v'/\partial x}),\text{ etc.}\right)$ which is always likely to negligible, and an eddy diffusion of eddy energy $\left(\frac{\partial}{\partial x}(\tfrac{1}{2}\,\bar{\rho}\,\overline{u'^2\,u'}),\text{ etc.}\right)$ which I Taylor (1952) finds to be negligible near the ground in a few cases investigated. result may not be generally true.

As little is known of the pressure-fluctuation term, Calder's analysis fails to to a usable criterion though it is of value in showing what terms additional to thos Eq. (6.3) may arise when a point criterion rather than a bulk criterion is sought.

Neither does Calder's treatment overcome the objection raised by Swinbank, it is clear that the terms $-g\,\overline{\rho'\,w'}$ and $\overline{w'\,\partial p'/\partial z}$ are not disconnected physically, the separation of them may prove artificial and misleading. Following text-b presentations of Richardson's criterion, Calder goes on to identify the former proportional to the vertical flux of heat. This would more properly apply to *eddy* flux $c_p\,\overline{(\rho w)'\,T}$ (see Section 9) for we can derive, without approximation, u the gas law, $p = R\rho T$,

$$c_p\,\overline{(\rho w)'\,T} = c_p\left(\frac{1}{R}\,\overline{w'\,p'}+\bar{w}\,\overline{\rho'\,T'}-\bar{T}\,\overline{\rho'\,w'}\right)$$

It is commonly accepted that $\frac{|w'|}{\bar{w}} \geqslant \frac{|T'|}{\bar{T}}$ and $\frac{|\rho'|}{\bar{\rho}} \geqslant \frac{|p'|}{\bar{p}}$. Applying these to right-hand side of the equation, the third term is seen to be of larger magnitude t the others, provided the correlation between ρ' and w' is not extremely small. T the formal identification is achieved. But the complement to the eddy flux is *mass* heat flux

$$c_p\,\overline{\rho w}\,\bar{T} = c_p\,(\bar{\rho}\,\bar{w}\,\bar{T}+\overline{\rho' w'}\,\bar{T},$$

containing an equal and opposite term, whence the total flux by bodily movemen

$$c_p\,\overline{\rho\,w\,T} = c_p\left\{\bar{w}\,(\bar{\rho}\,\bar{T}+\overline{\rho'\,T'})+\frac{1}{R}\,\overline{w'\,p'}\right\} = \frac{c_p}{R}\,(\bar{w}\,\bar{p}+\overline{w'\,p'})$$

This formulation does not however separate mass from eddy flux, as defined; the t in $\overline{w'\,p'}$ is a small part of the latter, and possibly of the total flux. We infer that general, mean and eddy motion are intimately related in the mechanism of heat flux: the assumption $\bar{w} = 0$ made by Calder and others is not acceptable (cf., footnote p. 5

It does not then seem reasonable to expect any criterion for the *eddying* ene entirely in terms of mean gradients or other easily measured properties of the f field but, guided by existing theories, practical tests have so far been directed to end. Paeschke (1937) has made observations on the growth of turbulence in surface layers of the atmosphere which he considers to be in general agreement w Schlichting's predictions. Durst (1933) on the other hand, from an analysis of conditi associated with the breakdown of nocturnal inversions near the ground, found evide in favour of a critical value of $Ri = 1$, though a very coarse estimate of the gradi was employed and this in a region where all terms on the right of Eq. (6.3) vary stron

with position : the first and third vary inversely with height (Calder 1949a, R. J. Taylor 1952) while Ri increases numerically almost linearly with height in statically stable and unstable conditions (Deacon 1949). These items of knowledge can however be put to little use in the equations and criteria of this section unless the variation of K_h and K_m with height is known. In an application of Richardson's criterion to the free atmosphere, Petterssen and Swinbank (1947) find 0·65 as the overall mean value for K_h/K_m; a smaller value would be appropriate if dissipation could be taken into account.

Although no single value of Ri is likely to determine the increase or decrease of turbulence, the number itself is evidently a valuable parameter for the correlation of turbulent flow phenomena in quasi-steady fields of varying static stability, (see, e.g., Deacon 1949). In that respect it is analogous to the Reynolds number which provides a correlation of phenomena over widely different speeds, scales of motion and fluid viscosity in geometrically similar fields.

7. *Some observed features of boundary-layer and free-air turbulence*

(a) *Profiles of mean quantities in the turbulent boundary layer.* Two distinct regimes of flow in a turbulent boundary layer have been identified, with corresponding velocity profiles and laws of drag. They are (a) *aerodynamically smooth* flow and (b) *aerodynamically rough* flow, that is smoothness or roughness of the boundary is defined, not geometrically, but in terms of the associated flow. Nikuradse (1932, 1933) made the classical investigations in each case, using circular tubes.

An aerodynamically smooth flow is one in which, by experiment, the velocity profile and surface drag are related to the fluid viscosity. For such regimes, in buoyancy-free flow,

$$\frac{u}{u_*} = \frac{1}{k}\log\frac{u_* z}{\nu} + \text{const.} \; ; \; z > \frac{30\nu}{u_*} \qquad . \qquad . \qquad . \qquad (7.1)$$

where u is the mean velocity at distance z from the boundary (we omit bars in this section), u_* is the *friction velocity* defined as $(\tau_0/\rho)^{1/2}$ where τ_0 is the surface drag per unit area, and k a universal constant (about 0·4, or rather less according to some recent work). The second constant depends somewhat on the geometry, e.g., circular pipe, channel, or flat plate. Thus a measure of velocity at a given boundary distance ($> 30\nu/u_*$) determines the surface drag. The appearance of ν in the profile equation and the lower limit to z imply the existence of a layer of fluid next to the boundary in which the shearing stress is progressively ' handed-over ' from turbulent to pure viscosity, and at the boundary $\tau_0 = \mu\,(\partial u/\partial z)_{z=0}$. The layer in which the shearing stress is predominantly viscous is called the *laminar sub-layer*. Its thickness ($\simeq 5\nu/u_*$) diminishes with increase in the velocity at a given boundary distance since u_* is nearly proportional to $u\,(z)$, from the profile equation. Since u_* is found to be of order 10^{-1} to $10^{-2}u$ when u is taken in the outer part of the boundary layer, then in air ($\nu = 1/7$) and for $u = 5$ m/sec the laminar sub-layer is of order 1 mm only !

An aerodynamically rough flow is one in which, again by experiment, the velocity profile and surface drag are independent of the viscosity but depend on a length which is related with the geometry of the surface roughness. Here, with buoyancy effects again excluded

$$\frac{u}{u_*} = \frac{1}{k}\log\frac{z}{z_0} \; ; \; z > z_0 \qquad . \qquad . \qquad . \qquad . \qquad (7.2)$$

where k is the same constant as in Eq. (7.1) and z_0 is the *roughness length* characteris of the aerodynamic properties of the roughness elements composing the surface. It generally one order of magnitude less than the height of the roughness elements; t factor depending on form and spacing (Schlichting 1936).

The absence of viscosity in the profile Eq. (7.2) implies that for such surfa the laminar sub-layer is absent and the drag arises from pressure forces associat with eddying wakes at the roughness elements. These eddying wakes do not fo until the local Reynolds number exceeds a critical value, so that a given surface m be aerodynamically smooth at a low rate of flow (roughness elements submerg within the laminar sub-layer) and aerodynamically rough at a higher rate. The criteri for the latter regime is $u_* z_0/\nu > 2{\cdot}5$.

Eq. 7.2 has been confirmed in the atmosphere by Sheppard (1947a) and Pasqu (1950a) from simultaneous measurements of surface drag and velocity profile, a that $u \propto \log z$ up to 10 m or more has been found by many workers. Thus a measu ment of mean wind at two heights can now be used to determine the surface dr and roughness length. (If the roughness is great the zero of height is not immediat evident and a zero-plane displacement must be determined empirically by making t profile conform with Eq. (7.2), a rational procedure since the datum level alwa falls appreciably below the top of the roughness elements.) Reasonably representat drags for crop-covered land surfaces are obtained by putting $z_0 = 0{\cdot}5$ cm to 10 (Deacon 1949) whence from Eq. (7.2), $u\,(10\text{ m})/u_* = 19$ to 11 and, with $u\,(10\text{ m})$ 5 m/sec, $\tau_0 = 0{\cdot}9$ dyne cm^{-2} to $2{\cdot}5$ dyne cm^{-2}.

It is more usual to relate the drag with wind speed u_a at anemometer level z_a means of the relation

$$\tau_0 = c\,\rho\,u_a^2$$

where c is the *drag coefficient*, a pure number. Then for rough surfaces, fr Eq. (7.2),

$$c = \frac{k^2}{(\log z_a/z_0)^2}$$

and ranges from 0·0028 to 0·008 for the above range of z_0 with $z_a = 10$ m. T synoptically more significant value of c for rolling, partly tree-covered country h been inferred by other methods to be about 3 to 6×10^{-3} (Taylor 1916, Sutcliffe 19 but a wind at anemometer height is hardly appropriate for reference and Eq. (7.2) evidently not applicable for the surface as a whole.

While practically all land surfaces are rough for any significant value of wi speed we do not yet know what profile obtains over the sea or how the drag depen on wind speed. According to Munk (1947) the surface undergoes a sudden chan of form at about 6 m/sec, c changing from 0·001 to 0·0026, but the evidence is co flicting (Charnock 1951a, Francis 1951, Montgomery 1951) and this very importa climatological quantity calls for intensive investigation.

In non-adiabatic conditions the wind profiles no longer obey Eq. (7.2). Deac (1949) has shown that over a land surface up to 13 m or so the general relation is

$$\frac{\partial u}{\partial z} \propto z^{-\beta} \qquad . \quad . \quad . \quad . \quad (7.$$

where $\beta \gtrless 1$ according as $-\frac{\partial T}{\partial z} \gtrless \Gamma$ and that very close to the surface the prof

s unaffected by the thermal gradient and tends to the adiabatic form (7.2) above. ntegration of (7.3) with this condition gives

$$\frac{u}{u_*} = \frac{1}{k(1-\beta)}\left\{\left(\frac{z}{z_0}\right)^{1-\beta} - 1\right\} \ldots . z > z_0, \beta \neq 1 \quad . \quad . \quad (7.4)$$

vhile for $\beta = 1$ we recover the logarithmic form (7.2). β is found to lie between bout 0·7 and 1·3 and for these extremes, and with $z_0 = 2$ cm, u (10 m)/u_* ranges from ·5 to 7 so that the drag for a given u (10 m) would be greater in the unstable case by a factor of 40. The comparison of drags for a given u (10 m) is however misleading, since the same wind at 10 m in a strong inversion as in a strong lapse would imply much greater free-air wind in the former case. For equal values of the free-air wind, he ratio of drags will be still greater than the above figure. This example serves to bring out a serious limitation of the empirical approach represented by Eqs. (7.1) to 7.4), namely that they contain no reference to the pressure gradient or free-air wind vhich, with other things equal, must determine the general level of wind speed near he earth's surface; in general the expressions do not converge as z increases indefinitely, nd cannot be used for extrapolation.

Deacon finds β for a given surface to depend only on the Richardson number (see Fig. 3). It should perhaps be added that very accurate observations of mean wind speed are required for its evaluation.

Profiles of other physical properties, e.g., potential temperature and specific humidity, are similar in form to the velocity profile over the same surface – see Sheppard (1947b) for the information then available. Later work has been done by Pasquill (1949) and by Rider and Robinson (1951), and the weight of evidence indicates that under lapse conditions there is a slight difference between the non-dimensional profiles of temperature and humidity in the lowest two metres. The temperature and velocity profiles certainly diverge at greater heights (Sutton 1949).

(b) *Properties of the fluctuations in the turbulent boundary layer and in the free atmosphere.* Complementary to the observations of mean profiles are observations of

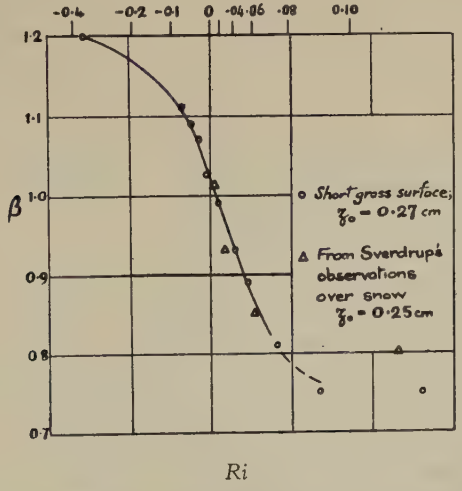


Figure 3. Dependence of the profile index β on the Richardson number. The abscissa (top) shows values of the latter obtained from mean gradients between 4 m and 0·5 m (after Deacon).

the turbulent fluctuations with which the profiles are associated. Hot-wire and oth techniques (cf., Goldstein 1938, Vol. I) were developed in the 1930s to provide t information for flow in the laboratory, and such research continues actively toda Less has been done in the atmosphere but the study is now properly receiving increas attention.

Of the laboratory investigations, that which has most relevance for meteorolo is probably the two-dimensional channel (width ≫ depth) on which Laufer (1950) h recently reported very fully. We reproduce a number of his results in Fig. 4 with t regret that it is not yet possible to provide a corresponding diagram for the atmosphe The figure speaks for itself, but we specially draw the reader's attention to a fe features. The first is the sharp maximum in $\tilde{u}'/U_0$ near the wall. This maximu occurs in fact *within the laminar sub-layer*, as is indicated by the corresponding fall the pecked curve of $-\overline{u'v'}/U_0^2$, whereas the total shear stress τ necessarily continu to increase right up to the wall. This feature might be looked for over the sea whe the laminar sub-layer may perhaps exist. The sharp maximum does not appear exist over a rough surface (Best 1935, p. 43). The second feature is the rapid ri

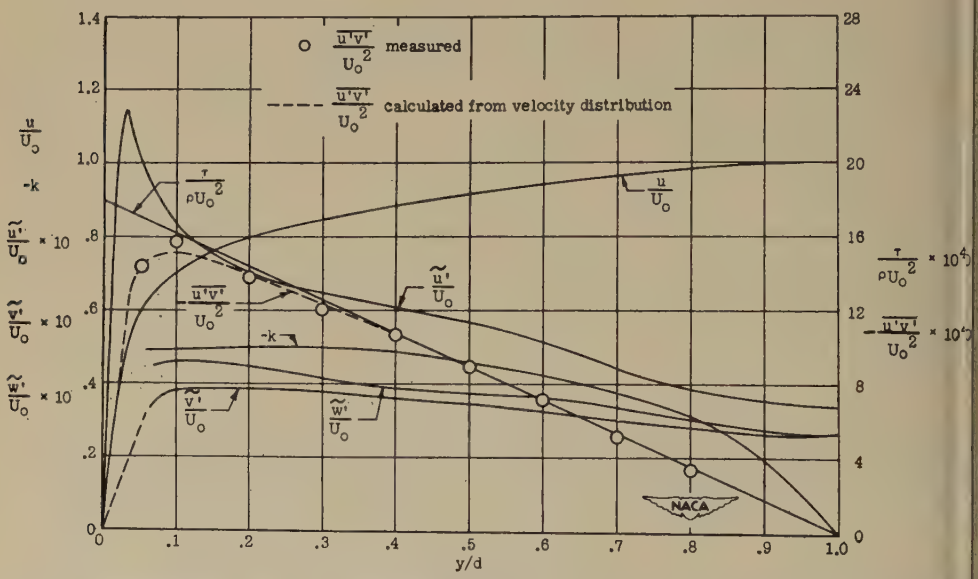


Figure 4. Distribution of mean and fluctuating quantities in a two-dimensional channel, $R = 12{,}300$ (af Laufer. Reproduced by the kind permission of the National Advisory Committee for Aeronauti Washington, D.C.).

U_0 = mean velocity at centre of channel.

u = local mean speed at position y/d where y = wall distance and d = half width of channel. (T given curve is well represented by $u/u_* = 6{\cdot}9 \log_{10}(u_* y/\nu) + 5{\cdot}5$ for $30 < \frac{u_* y}{\nu} < 400$).

$\tilde{u}', \tilde{v}', \tilde{w}'$ = root-mean-square velocity fluctuations, (i) along the channel, (ii) in the y-direction (i. normal to wall) and (iii) parallel to the wall at right angles to the mean flow, respective (N.B. We have used a different convention elsewhere in this paper, using w' where Laufer us v' and vice-versa).

τ = shearing stress (from the equation of mean motion $\tau = \tau_0(1 - y/d)$ where τ_0 is the shear stress at the wall, given by the pressure gradient, or else by $\tau_0 = \mu(\partial u/\partial y)_{y=0}$).

k = coefficient of correlation between u' and v' on which eddy shearing stress depends.

of $\overline{v'}/U_0$ with distance from the wall to reach a value for $y/d > 0{\cdot}1$ which is roughly equal to $\overline{w'}/U_0$ and about two thirds or less of $\overline{u'}/U_0$. Thirdly, we note that the turbulent shear stress arises from a variance in v' of which only a quarter or less is 'dependent' on u' ($-k \leqslant 0{\cdot}5$).

The production of turbulent energy in the channel outside the laminar sub-layer is immediately available from τ and $\partial u/\partial y$ (Section 6) and Laufer also measured the viscous dissipation. The production of energy is greater than the dissipation for $0{\cdot}2\,d < y < 0{\cdot}7\,d$ and less beyond $0{\cdot}7\,d$ — it is necessarily zero at the centre – so that energy is diffused towards the middle regions. Finally, Laufer showed that the turbulent shear stress arises from the larger eddies only (at $y/d = 0{\cdot}4$), which accounts qualitatively for the rather small correlation between u' and v'.

Such data of this sort as we possess for the atmosphere are mainly due to Scrase (1930), Giblett *et al.* (1932), and Best (1935), and latterly to Swinbank (1951a, b, see also Section 11), but for the lower part only of the boundary layer. The behaviour here is much complicated by the highly variable temperature gradient, and at higher levels by Coriolis effects and by the variation of mean wind with height due to horizontal temperature gradient (Section 16). The fluctuations of temperature in the lower atmosphere are now receiving attention as evidenced by recent work by Gerhardt and Gordon (1948), Shiotani (1950), Sheppard (1951a) and by R. J. Taylor (see Priestley 1951c). No coherent picture can however yet be presented.

Information regarding the smaller-scale turbulence of the free atmosphere is also scanty but Durst (1948) has reported pioneer work, using smoke puffs, on fluctuations from a few minutes to an hour in period and on a horizontal scale of order 1,000 ft, at heights between 2,000 and 20,000 ft. He infers that turbulence on this scale is of the same order throughout the layer though diminishing somewhat in intensity with height (in clear weather). Another promising line of investigation is the measurement of temperature and humidity fluctuations in the free air from aircraft (Grant 1952) and by means of stellar and radio scintillation (Megaw 1950, Butler 1951, Ellison and Seddon 1952). Clear-air turbulence, sometimes intense, in the region of jet streams or upper-air troughs has recently attracted considerable attention on account of its importance to aviation. Its relation with Ri and jet-stream structure has been investigated by Bannon (1951, 1952).

III. Mechanism and measurement of transfer

8. *The use of models*

As stated, the practical importance of turbulence lies largely in the transfer which it effects. No basic theory of such transfer having yet emerged, recourse has been made to 'model' theories which, provided their inherent limitations are recognized, may still be a valuable guide to thought and observation. A question which they bring strongly to our notice is the extent to which the main feature of molecular transfer, i.e., transfer down and proportional to the gradient of the significant property, may hold also for turbulent flow.

9. *Formulation of eddy flux*

To formulate the expression for the flux across a fixed surface σ of some property whose measure per unit mass is denoted by s, we must consider the totality of elements whose turbulent motion carries them across the surface. If, at the instant of crossing,

an element has density ρ and velocity v normal to the surface, it will be effectin transfer of the property at the rate ρsv per unit area. The net flux is then the averag of this quantity over all the elements to be considered, i.e., over those elements crossin the surface within a space-time domain appropriate to the problem in hand.

Reynolds's attempt to provide an exact formal treatment in terms of *continuou* averages has been seen to fail within a turbulence spectrum which is unbroken. Th advantage of continuity of the average is, however, a doubtful one, at any rate whe dealing with concepts such as eddy transfer which have no meaning at a given poin and instant of time. The convention which we propose to adopt sacrifices continuit of the average for the advantages of unambiguity of definition and separability betwee mean and eddy properties provided that the space-time domain be specified; th domain is no more arbitrary than the Reynolds interval τ. The connotation is th same as Reynolds's but need not be confused with it.

The elements crossing a specified area of surface within a specified interval hav a unique average value $\bar{s}$ of s. Again we shall write $s = \bar{s} + s'$ and refer to the prime quantities as anomalies. With this convention lemmas (3.2) and (3.3) are alway satisfied; (3.1) has no longer any significance, but a similar relation holds for th corresponding finite differences. The *eddy flux* is then defined as

$$F_{s,\sigma} = \overline{(\rho v)' s} \quad . \quad . \quad . \quad . \quad (9.1$$

$$= \overline{(\rho v)' s'} \quad . \quad . \quad . \quad . \quad (9.2$$

This excludes from the concept of eddy transfer the *advective flux* due to the mea mass flow across the area in the time considered, which may in turn be regarded a due in part to eddies whose dimensions are comparable with those of the domain. is from these latter, as we have seen, that the error in Reynolds's formulation arise The present approach avoids any formal difficulty, but we must bear in mind that th eddy flux may increase (or decrease) as the domain increases.

Expressions (9.1) and (9.2) are applicable to the flux of any property, irrespectiv of whether it is a property of the element which in turbulent motion is only change by mixing (so-called conservative properties) or one which may change through th motion (e.g., sensible heat) or through some concomitant of the motion such as th action of fluctuating pressures (e.g., momentum).

The macroscopic equations to which considerations of eddy transfer may b applied must, with the above convention, be in terms of finite differences rather tha differentials – this appears to us but an academic point and in practical applications is in fact normally so. But it is not possible to express too strongly the importance ensuring consistency between the space-time averaging of the eddy terms and th definition of the macroscopic or ' mean ' condition. Cowling and White (1941) hav discussed examples of the errors arising when this precaution is overlooked.

10. *Mechanism of transfer*

Classical developments centred on the attempt to express Eq. (9.1) in terms the macroscopic conditions, which could be more readily measured or appreciate The work of Schmidt and Taylor led to the formulae

$$F_{s,\sigma} = - A_{s,\sigma} \frac{\partial \bar{s}}{\partial n} \quad . \quad . \quad . \quad . \quad (10.$$

or approximately

$$F_{s,\sigma} = -\bar{\rho} K_{s,\sigma} \frac{\partial \bar{s}}{\partial n} \quad . \quad . \quad . \quad . \quad . \quad (10.2)$$

where $\partial/\partial n$ represents differentiation along the direction of the normal to σ in the sense to which the flux relates. (Where the meaning is clear, the suffixes to the F, A, K will be dropped; most commonly σ will refer to z, the horizontal plane). A is known as the *Austausch* coefficient and K alternatively as the eddy viscosity (K_m), eddy conductivity (K_h) or eddy diffusivity (K_v when applied to water vapour). They are defined by

$$A = \overline{(\rho v)' l}, \quad K = \overline{v' l} \simeq A/\bar{\rho}$$

where l is a quantity having the dimensions of length. The theory leading to Eqs. (10.1) and (10.2) has become known as the *mixing-length theory*, although the expressions can be derived without using the concept of mixing. Priestley and Swinbank (1947) argue that a relation between the individual v' and individual l admitting of sensible interpretation only exists if l is defined as the distance moved by the element in the direction of n since it last had no component motion in that direction. It then follows from the assumptions of the classical model, viz., that s is only changed by mixing, that K is a necessarily positive function determined uniquely by the structure of the flow pattern and would therefore be the same for any conservative property under consideration. Further advance then depends on relating l to the geometry of the flow (Prandtl) or to the properties of the mean motion at the point considered (von Kármán), leaving a constant to be determined by experiment.

It is admissible to adopt a purely dimensional approach and write down Eqs. (10.1) and (10.2) regarded as defining equations for A and K. There is however no *a priori* justification for assuming the flux to depend on the gradient alone among the features of the macroscopic field, and it must therefore be shown that the K's so defined do possess certain regularities of behaviour. The method is of little physical content but has the virtue that it allows reference to effective eddy coefficients, and possible differences between them, without any prejudice as to model or conservatism; advantage of this has been taken from time to time in the literature, and the practice will be followed here.

Another assumption implied in the classical model is that the turbulence is indiscriminate, or that the motion of an element is unaffected by its individual properties or individual environment. With regard to the former, it has been shown by Ertel (1942) and Priestley and Swinbank (1947) that the flux should be written

$$F_{s,\sigma} = -\bar{\rho} K_{s,\sigma} \frac{\partial \bar{s}}{\partial n} + \overline{(\rho v)' s''} \quad . \quad . \quad . \quad (10.3)$$

where s'' represents the anomaly at the instant defining the initial point of l. The additional term represents a selective transfer whose magnitude will depend, not on the mean gradient, but on the extent to which the s-fluctuations may themselves affect the flow or on the size and scale of the fluctuations and the mechanism sustaining them; in few if any cases in atmospheric turbulence can it be asserted that these fluctuations are entirely due to the turbulent motion itself. In the case of vertical transfer of heat near the ground, the two terms on the right are conveniently referred to as *mechanical* and *convective turbulence* respectively. The physical inhomogeneity of the ground must itself give rise to temperature anomalies in the air, and we are led to expect a

K larger for heat than for other properties in superadiabatic and smaller in inversion conditions, by amounts which depend on the relative strengths of the influences at work. A possible consequence of selective transfer is that the 'neutral' condition, i.e., the condition for no transfer, may differ from the zero gradient of s, implying the existence of a range of conditions in which A and K as defined by Eqs. (10.1) and (10.2) would have negative values. The view that this is not a suitable formulation of the problem has led to a search for properties whose gradients are more closely associated with the transfer (Montgomery 1948). It does not seem possible at present to resolve these questions by mere argument since, though buoyancy is the most obvious selective influence, the intercorrelations between different properties including the micro-fluctuations of pressure are complicating factors whose effect requires further study.

The second possibility of selective transfer is suggested when we come to analyse the individual environment in terms of different space-time scales. It is usual to assume that the eddying activity is determined by the macroscopic features; for example activity near the ground depends on vertical temperature gradient and wind shear in that region (cf., Section 6), activity on the synoptic scale on the broad lateral (or isentropic) and vertical wind shear, and so on. But it is not known at what point in the scales of space and time these influences are most strongly obeyed. To illustrate the point, one might argue from Eq. (10.2) that over a period of 24 hours the vertical heat flux near the ground is small because the mean diurnal temperature gradient is small. In fact the net flow is upwards and the magnitude considerable because K_H during the hours of lapse is many times larger than during inversion; in more general terms, the activity is determined by conditions on a shorter time scale than the 24 hours of the macroscopic process. This is reflected in the practice of quoting values of K as applicable only to much shorter periods, but one might with equal justification have argued that selective influences are at work on the 24-hour scale. Coming now to the shorter periods themselves the same alternatives remain, for variations in gradient exist on the scale of centimetres and the period of seconds, and these variations in turn may well control the eddying activity at that scale so causing a selectivity which would only cease to be apparent, as such, if the space-time scale were reduced still further. The central difficulty, that of averaging Eq. (10.2) which is in reality non-linear, can only be progressively overcome as knowledge of the physical structure and its interaction with the flow pattern penetrates further into the small scale.

11. *Measurement of transfer*

Increasing awareness of the shortcomings of Eqs. (10.1) and (10.2) has turned attention towards the possibilities of the direct measurement of transfer from expressions (9.1) and (9.2).* This involves effectively simultaneous and coincident determinations of ρv and s with checks on the adequacy of the resolving power of the instruments. There are two fields of problems, or scales of eddy transfer, at which such determinations are now being made.

Preliminary measurements of the vertical flux of heat, water vapour, and momentum at $1\frac{1}{2}$ m above ground have been made by Swinbank (1951a, b). The few results so

* In some problems it is the total flux $\overline{\rho v s}$ rather than the eddy flux which is required, but where the technique is limited to measurements at a single point the latter is the best that can be obtained owing to the impracticability of measuring a representative value of $\overline{\rho v}$. The flux due to 'standing eddies' and to the overall mean mass motion must be otherwise estimated or obtained by a sufficient number of sets of measurements distributed over the area concerned.

far reported were consistent in indicating that in strong lapse conditions the values of K for heat, momentum, and water vapour were in the ratio of approximately 4: 2: 1 whereas under strong inversion conditions approximate equality was approached. The measurements were made over a flat, rather thinly grassed paddock and can only be applied with reservation to other sites owing to the possible influence of ground inhomogeneity already referred to. The need for similar determinations at a variety of sites, especially over water where additional resolving power might be required, parallels the need for continued investigation over land to refine the results in terms of wind speed, height and other factors. Within the limits set by different sites, levels, etc., the findings with respect to the heat-vapour comparisons were in agreement with those inferred by Pasquill (1949) from measurements of profile and energy balance. On the other hand, disparity is to be noted in the comparisons involving K_m under inversion conditions; Pasquill's indirect method is here the more suspect of the two, though Swinbank's results certainly require confirmation.

The second example of direct use of Eq. (9.1) is to be found in papers by Priestley (1949, 1951a), Widger (1949) and others concerned with horizontal transfers through the whole depth of atmosphere on the global or continental scale. The applications and, in general, the results are not relevant to the scope of this review, but two aspects may be noted as significant. The first is that on this scale the relation between the horizontal transfer and gradient is not in general to be reconciled through a coefficient which is also a measure of the activity. Though one may follow Defant's (1921) original conception of a *Grossaustauschkoeffizient* by writing

$$F_{s,\phi} = -\rho K_{s,\phi} \frac{1}{a} \frac{\partial s}{\partial \phi}$$

where ϕ represents latitude and a the radius of the earth, the structure of the transporting agents is such that the coefficients, beyond conforming normally to an order of magnitude ($K \simeq 10^{11}$), have little physical significance. For example the meridional eddy transfer of angular momentum increases with height through the troposphere in sympathy with the intensity of activity, but the corresponding exchange of sensible heat does not. Moreover in the stratosphere strong eddy transfers of heat polewards, i.e., from cold towards warmer regions, have been measured. Nor does it appear that the transfer of angular momentum is everywhere polewards, although the direction of the mean gradient is in that sense. The possibility for selective influences is clear enough, and one task for research is to identify them.

The second significant feature of the large-scale work is the relatively small range of sizes of eddy contributing to the transfer. It has been found (Priestley 1949) that variations finer than one day make no important contribution, and it is a reasonable inference that variations over a length small compared with the size of anticyclones and depressions (say 100 mi or so) can likewise be disregarded.

A similar restriction of range appears in vertical transfer near the ground (we have already noted the evidence from the laboratory in Section 7). Panofsky (1951) finds the vertical transfer of momentum at 109 m to be dominated by variations of between about one and ten minutes. His material was confined to superadiabatic conditions when convection-dominated turbulence might be expected. Nevertheless his lower limit is in line with a criterion proposed by R. J. Taylor (1951) from studies of wind-speed fluctuations at lower levels; namely that the period above which eddies cease to be isotropic (see Chapter VI), and so are able to contribute to the stress, is

approximately equal to the height above ground divided by the wind speed. Whi
the theoretical validity of Taylor's criterion, and of Panofsky's variant, is admitted
open to question, there can be little doubt that the order of magnitude of their resul
is correct and that their papers represent pioneering efforts in what should prove a
important line of enquiry. From Swinbank's results the way is open for a paralle
investigation of the spectra of the heat and vapour transfers.

It is suggestive that at two extremes of the scale is found a similar restriction an
that the limiting size or period of eddy bears broadly the same ratio to the scale
the corresponding macro-phenomenon. In both cases fluctuations of shorter perio
are present but are not so correlated as to be important in transfer. If, as is a reasonab
speculation, the same holds for those intermediate-scale transfer problems which hav
not yet been subjected to direct attack, we are provided with a useful guide as to th
resolving power required from the instruments or techniques yet to be develope
These problems include many of great importance to the future advance of meteorology
such as the macro-physics of clouds, the patchiness of clouds and of clear air, frictio
and the vertical transfer of heat associated with convection and with motion on th
synoptic scale. But while a start has been made as in the work of Durst and Gra
already noted (Section 7b) the lack of sufficiently exact knowledge of the simultaneou
velocity-component and temperature fluctuations has so far prevented any quantitativ
estimate of the transfers of heat and momentum.

12. *Indirect evaluation of eddy transfer*

Although the present need is clearly for more direct and detailed measurements,
considerable body of knowledge on the magnitude and variation of the eddy transfe
in the near-surface layers has been built up by less direct methods. This has bee
described by Sutton (1949) and repetition is not required though discussion of one o
two points would be appropriate.

It has been shown by Ertel (1933) and Calder (1939) that if there is no acceleratio
and the wind turns only very slowly with height the horizontal shearing stress τ (c
vertical flux of momentum) is almost independent of height up to at least 30 m. Thi
allows the variation of K_m with height to be derived from the observed wind profile
Thus from Eqs. (7.2) and (10.2), $K_m = ku_*z$ or, if the profile can be represente
as a power law in z, K_m appears as the conjugate power, a tractable form of variatio
which allows ready mathematical solution of certain problems in diffusion. Howeve
the constancy of τ with height has not yet been confirmed by observation. Th
much-quoted measurements of Scrase (1930) indicating a fourfold increase in τ betwee
1·5 and 19 m over downland are open to suspicion through lack of simultaneity of hi
records and of resolving power of his instruments.

One possible consequence of the change in τ, and hence of the ' equilibrium
profile, with roughness has received little attention. Over land it is the exceptio
rather than the rule for the roughness to be constant over large areas; and while it i
valid to refer to the representative condition of the grass or close cover when discussin
the wind at a few metres, the wind at higher levels must be influenced by trees, hedge
etc., and the broader aspects such as contour. The concept of the equilibrium o
completely adjusted profile, which underlies the work of Ertel and Calder, may i
practice be seldom realized over land. The constancy of stress is at present, therefore
at best a working hypothesis. However it is assured that the mean height-variation

f any, is at a slower rate than that of the wind shear, so the inference that K_m increases apidly with height in the surface layers remains valid.

Sutton (1948) has shown that the flux of heat may be taken as constant up to 100 m over land during the midday hours in clear summer weather with light winds. Through these levels the temperature lapse rate decreases with height more rapidly than the wind gradient so that, assuming constancy of stress, K_h increases more rapidly than K_m, implying a progressive strengthening of the role of convective turbulence; the manner in which this is accomplished can be only understood from further studies in micro-structure. In more general conditions the constancy of heat flux is a less good approximation though it does appear, together with a similar constancy of vapour flux, to hold generally over height intervals of 2 m or so (Pasquill 1949; Rider and Robinson 1951) so that a rapid increase with height of the K's appropriate to all elements emerges as a general law. Under stable conditions the increase may give way to a decrease at about 50 m (Booker 1948).

It has been argued in more than one place that similarity of profiles for different elements implies equality of K's. It is clear from Eq. (10.2) that this follows only when the fluxes of the corresponding elements also obey identical laws of height variation, and even then the corollary is strictly that the ratios of the K's remain constant with height. In several instances where claims for equality of K's have been made from profile data, the conclusion would more properly have been made that the ratio between the K's changes with height much more slowly than their absolute values.

It has long been known, from a variety of evidence, that the absolute value of all the K's depends strongly on the degree of thermal stratification. Near the ground Pasquill's results indicate a variation with the Richardson number of about twelve-, six-, and eighty-fold in the K's for heat, vapour and momentum respectively, though the last is the most uncertain owing to the assumptions involved. Absolute values (at 75 cm) ranged from 2,200 to 240 cm^2/sec (K_h), 1,400 to 200 (K_v) as compared with Swinbank's (at 150 cm) of respectively 2,220 to 35, 500 to 35, and 1,200 to 45 (K_m). As Swinbank's measurements extended into rather lighter winds in the stable conditions the agreement is reasonably good, though more work is required to establish representative values and to decide whether specification at any height in terms of the Richardson number or some other simple function of wind and lapse rate is sufficient. Lettau (1941), applying Eq. (13.3) of the next section, has derived values of K_h at 45 m from 10^3 to 2×10^5 while Sutton (1948) quotes values as high as 5×10^5 at comparable levels. The enhanced range at higher levels reflects the effect of stability on the variation of K with height, the relative increase being more rapid in the unstable condition.

Little is at present known of the absolute magnitude, and variation with height, of the horizontal transfer co-efficients K_y (across wind) and K_x (downwind) which become of importance in many problems of diffusion and evaporation. Relevant information from the results of diffusion experiments has lain idle through lack of a satisfactory solution through which to interpret it.

IV. Application of transfer theory

13. *Scope of application; the differential approach*

G. I. Taylor first applied his theory to the problem of fog formation when warm air moves over a cold sea. Assuming K_h constant, for an order of magnitude determination,

the cooling may be shown to extend, after time t, effectively to a height $2(K_h t)^{1/}$ From measurement of the vertical temperature distribution Taylor found, for th stable stratification, values of K_h of 10^3 and 3×10^3 cm^2 sec^{-1} for winds of force and 3 respectively.

Most applications have been concerned with the detailed behaviour of shallo rather than the relatively broad behaviour of deep atmospheric layers; that is to sa to differential rather than integral problems. The equation involved is normally variant of the simple one-dimensional equation of transfer

$$\frac{\partial}{\partial t}(\overline{\rho s}) = -\frac{\partial}{\partial z}\left\{\text{total vertical flux}\right\}$$

$$= -\frac{\partial}{\partial z}(\overline{\rho w s}) \quad . \quad . \quad . \quad . \quad . \quad (13.$$

Expanding, using the equation of continuity $\partial\bar{\rho}/\partial t = -\partial(\overline{\rho w})/\partial z$, neglecting $\partial(\overline{\rho' s'})/$ and writing $\bar{\rho}\frac{d}{dt}\left(\equiv \bar{\rho}\frac{\partial}{\partial t} + \overline{\rho w}\frac{\partial}{\partial z}\right)$ for differentiation following the mean ma motion,

$$\bar{\rho}\frac{d\bar{s}}{dt} = -\frac{\partial}{\partial z}\left\{\overline{(\rho w)' s}\right\} = -\frac{\partial F_{s,z}}{\partial z} \quad . \quad . \quad . \quad (13.$$

or, using Eq. (10.2)

$$\bar{\rho}\frac{d\bar{s}}{dt} = \frac{\partial}{\partial z}\left(\bar{\rho} K_s \frac{\partial \bar{s}}{\partial z}\right) \quad . \quad . \quad . \quad . \quad . \quad (13.$$

In a large number of problems the difference between $\partial/\partial t$ and d/dt has bee disregarded, while in those which are not one-dimensional, additional terms appe due to horizontal advection and horizontal diffusion.*

It is thus not only the absolute value of K but also its variation with height which of importance. Provided these are known, or can be assumed, the solution of an specific problem depends only on the initial and boundary conditions and mathematic skill, or the availability of computing machines.

A classical example is the decay of amplitude and lag of phase with height of th diurnal temperature variation. Detailed studies of this problem are given in th textbooks and also in papers by Cowling and White (1941) and Lettau (1941). Broadl the results agree in indicating a K of order 10^5 cm^2 sec^{-1} for the lowest 100 m, but was early noticed by Chapman (1925) that the temperature variation cannot be entirel explained by conduction on the K-model. Some of the discrepancies may be due t the complexities discussed in the previous section, while near the ground the effe of long-wave radiation is by no means negligible (Robinson 1950).

In the two applications so far referred to the analysis has been confined specificall to the flux, or eddy transfer coefficient, appropriate to the property considered. I other problems, through lack of proper data progress has only been made by identifyin one K with another in terms either of its absolute value, or of the form of its variatio with height, or both. The desire to reconcile in some measure atmospheric wit

* Eqs. (13.1) to (13.3) are equations of conservation and do not, without modification, apply to entities which are convertibl e.g., heat or turbulent kinetic energy. For the complete equations of thermal energy the reader is referred to van Mieghem (194 pp. 46, 47); we note here only that the result given in Section 6, that the total flux of heat by bodily movement is an intima compound of mean and eddy vertical motion, is probably related with convertibility and the terms arising therefrom and ca for study.

boratory results and the belief in the law of constant stress have combined to make $_m$ the coefficient most often appealed to in this manner, despite the importance of ermal stratification. Commonly a wind profile is assumed to be known and the $_m$ derived therefrom substituted for the K_s of the problem in hand. The uncertainties volved in this procedure have already been referred to. On the other hand it must borne in mind that in the surface layers the differences between the K's is likely be, in general, quite small compared with the variations with wind, time of day, d height above ground, as is borne out by the evidence already cited. So there may ll be a large field of application in which the procedure is valid, at least as a first proximation, particularly as it is frequently a relative rather than a precise absolute sult which is required. In any case it is necessary to deal in broad outline with ese applications, since it is to them that the main effort has in the past been devoted.

14. *Evaporation*

Thornthwaite and Holzman (1939) have given an expression for evaporation per nit area of the form

$$E = \rho k^2 \frac{(u_2 - u_1)(q_1 - q_2)}{(\log z_2/z_1)^2} \quad . \quad . \quad . \quad . \quad (14.1)$$

hich is readily derived from Eq. (13.3), setting the left-hand size to zero, and using the garithmic law (7.2) of wind structure to provide K_v, and allows the evaporation to calculated from measurements of wind speed and specific humidity q at two heights ove the ground. Pasquill (1950b), properly modifying the expression to allow for e zero-plane displacement and therefore requiring wind measurement at a third ight, found that the formula agreed with measurements of evaporation from small ass-covered plots under conditions when the log law is applicable. There is no stification for applying the formula under lapse or inversion conditions.

The purely one-dimensional treatment above assumes the nature of the surface be uniform, in a statistical sense, and is unsuited to the study of evaporation from eas where there is a transition or discontinuity in surface and the evaporation per nit area varies with position. One must then include the advective term $\overline{\rho u}\,\partial/\partial x$ on e left-hand side of Eq. (13.3) and so allow for the progressive downwind development a new vapour boundary layer over the area under study. For discrete damp surfaces dry surroundings solutions have been given by Sutton (1934), Jaeger (1945) and hers, using a power-law variation of wind appropriate to flow under neutral nditions over an aerodynamically smooth surface. Sutton's solution is of the form

$$E \propto u^{0\cdot78}\, x_0^{0\cdot89}$$

being the downwind dimension of the damp area. The indices agree with those und in laboratory experiments. Calder (1949b) has given a more satisfying treatment r flow under neutral conditions over smooth surfaces and extended it to rough rfaces, approximating the logarithmic wind relation by a power law to give tractability the mathematics, with the result for the rough surface

$$E \propto u\, x_0^{0\cdot89}$$

which the dependence on wind speed echoes that in the simpler expression (14.1).

This would apply to land surfaces, but whether to an open water surface depends a resolution of the aerodynamic nature of such a surface.

The bulk of hydrological evidence of evaporation from enclosed water bod does in fact support a wind index less than unity. On the other hand the advantag of considering the advective term are to some extent offset by the fact that exce in arid regions, the surroundings of a lake are seldom completely dry. Moreover if t discontinuity is sufficient to make the effect of a developing *vapour* boundary lay important, similar developments will occur in the wind and temperature structure al features which existing theories fail to recognize. The effect of the transition temperature and roughness on the evaporation awaits solution in quantitative terms.

15. *Diffusion*

The most intensive application of transfer theory has been to problems of atmo pheric diffusion in the surface layers. The work of Roberts and of Sutton (194 1949), provided a solution for the concentration downwind of a continuous infin line source in which the decay of concentration with distance was correctly predict as $x^{-0.9}$, but the absolute value and the height of the cloud were in error. Calc (1949b) showed that this discrepancy was due to the assumed ' smooth ' law of wi structure, and has given a more acceptable solution based on the rough-surface prof under adiabatic conditions. Absolute values from this solution agree with the resu of trials at 100 m downwind of the source, and the decay with distance is broadly t same as in Sutton's earlier result though it depends somewhat on the surface roughness

For prediction of diffusion under other than adiabatic conditions Deacon (194 has employed his generalized wind profile (Eq. 7.4) from which, since $\tau = \rho K \partial u/\partial z$,

$$K = k u_* z_0 \left(\frac{z}{z_0}\right)^{\beta}$$

where z_0 and β are obtained from the profile. Broad agreement was then obtain with the results of a limited number of trials under unstable conditions. Before t general adequacy of the Calder-Deacon solutions can be judged, more experimen are required under extreme conditions, and these should be on a large scale sin extrapolation to much greater distances of travel is uncertain for reasons which w appear in the following paragraphs.

Three-dimensional diffusion, e.g., from an instantaneous (puff) or continuo point source, has not yet been satisfactorily treated by transfer methods, partly becau of the mathematical complexity but principally because, lacking a physical counterpa to the law of constant stress, we remain in ignorance of the proper functional fo of the coefficients of lateral eddy exchange K_x, K_y. The evaporation from an ar of limited extent across wind admits the same difficulties.

The complete equation for the concentration χ usually invoked is

$$\frac{\partial \chi}{\partial t} + u\frac{\partial \chi}{\partial x} = \frac{\partial}{\partial x}\left(K_x \frac{\partial \chi}{\partial x}\right) + \frac{\partial}{\partial y}\left(K_y \frac{\partial \chi}{\partial y}\right) + \frac{\partial}{\partial z}\left(K_z \frac{\partial \chi}{\partial z}\right) \quad . \quad . \quad (15.$$

All the evaporation and diffusion solutions referred to above are essentially solutio of this equation in one of its simplified forms. The full equation has only been succes fully handled by writing $K_x = K_y = K_z$, for which there appears no physical justificati and treating them as constants. Even then agreement with observation was on obtained by invoking an absolute value for K which increased with distance from t

ource, or with the scale of the phenomena. Such a procedure is mathematically ndefensible, though allowance for the effect of scale, if invoked in a consistent manner, as some physical justification, as was early advocated by Richardson (1926) (cf., Chapter V also). Lettau (1951) has pointed out that Eq. (15.1) also neglects the romotion of diffusion through the maintenance of sharp gradients by wind shear Eckart (1948) in a parallel problem in oceanography makes an interesting distinction etween stirring and mixing). Lettau shows that an additional term involving $\frac{\partial u}{\partial z}\frac{\partial \chi}{\partial z}$ hould appear, whose effect would be similar to an increase of K with distance. The ffect would clearly be most significant with instantaneous or short-time sources nd it would be interesting to see to what extent the scale factor varies as between n instantaneous and a continuous source. Notwithstanding these complications, the traightforward solution of Eq. (15.1) with K_x and K_y varying as arbitrary powers f z alone would represent a useful theoretical advance and, with observations of iffusion from point sources, allow at least the relative magnitudes of K_y and K_z to e established.

16. *Flux at a boundary; the integral problem*

In the examples given above the emphasis has been on the study of relatively hallow layers of the atmosphere. The essential feature of these problems is that they re of a differential nature, resting, as Eqs. (13.1) and (13.2) imply, on the often elatively small changes of the flux with height rather than the absolute value of the lux itself. Progress has been due largely to the circumstance that the investigator s able to measure a profile, and so obtain direct knowledge of macroscopic changes with time and place. From this aspect it may be seen that the only quantities in Eqs. 13.3) or (15.1) not directly known or approximately measurable are the K's themselves; hese then become the principle unknowns and some of the solutions, presented as pplications, might rather be regarded as formulae whereby, with necessary observa- ions, to obtain light on the turbulence processes themselves, becoming applications nly when they are subsequently used for interpolation or extrapolation.

The situation is in several respects quite different in what might be termed the ntegral problems, which have in the past received relatively little attention from the urbulence specialist but which are of daily concern to the meteorologist in a wider ense; or, rather, would be so if knowledge of transfer processes were further advanced. The governing equation becomes, from Eq. (13.1),

$$\int_0^\infty \frac{\partial}{\partial t}(\rho s)\, dz = \text{total flux at boundary} \qquad (16.1)$$

which has, on its left side, an expression not readily open to direct measurement. On he other side we are now concerned, not with differences, but with the absolute value f the whole flux into the vertical column. For practical purposes, as has been seen, we may regard this as applying to the value of the flux at one metre or so rather than t the surface itself.

This may be written as

$$F_{s,z} + \overline{\rho w}\,\bar{s} \quad \text{or} \quad -\bar{\rho} K \frac{\partial \bar{s}}{\partial z} + \overline{\rho w}\,\bar{s}$$

The second term, where it is important, must be inferred from considerations of

continuity, the first from measurements or knowledge about the turbulence. Whe in differential problems it has been a common custom in the past to ignore poss differences between the K's on the basis that these are generally much smaller t the space and time variations, it is evident that even (say) a twofold difference betw one and another cannot be overlooked in any problem which is governed by Eq. (1 rather than (13.3). Subject to Eq. (16.1) are major synoptic problems of air-m modification, and larger-scale questions of atmospheric energy, water, and angu momentum balance.

The pioneer work in this field is that of G. I. Taylor already noted. More rece there has been important work by Jacobs (1942, 1949) and Albrecht (1949), who h located, on a climatic basis, the principal regions of energy exchange between oc and atmosphere. On the other hand Sverdrup (1945a) and Craddock (1951) h evaluated air-mass transformations of unstable air – see also Collman (1951) – w Craig and Montgomery (1949) have made similar but smaller-scale evaluations humidity modification in stable air. For the climatic studies the reader is referre maps appearing in Jacobs's papers or in Sverdrup (1945b). They are based on accur lated data on sea-surface temperature and temperature, humidity, and wind in air above, leading to values of the gradients and hence of the transfers, based of neces on the assumption of equal K's and a wind-stress relation of approximate form. Thou the absolute values must therefore be regarded as uncertain, the broad features of patterns are likely to be correct, and these in themselves constitute an applicatio major importance to meteorology. No parallel accuracy for the land regions is yet possible. There is a number of contributory reasons for this, but it is clear t the lack of exact knowledge of the absolute values of the K's has been a main bar to progress in the whole of this field. The case-history method overcomes this deficie by using observed values of temperature and humidity in depth along the trajecto assuming however the absence of mean vertical motion. It is found that the turbul energy exchange between the ocean and polar air may be about 30×10^{-3} cal cm^{-2} se (roughly the solar constant) over many hours. In stable air the transfer in latent fo may be about a twentieth of this in the first few miles of travel over the sea.

Swinbank's measured values over land at $1\frac{1}{2}$ m reach 3×10^{-3} cal cm^{-2} sec^{-1} the upward eddy flux of heat in sensible and 5×10^{-3} in latent form. There rema no room for doubt about the importance of both these contributions not only in mo fication of the surface layers but in the deeper process of air-mass transformation a in the total energy balance of the earth and atmosphere. The amounts transfer downwards are a magnitude smaller, so that in climatic applications over land c must be taken to weight the observed gradients heavily in favour of the midday hou Further potential applications of the absolute flux determinations extend into t adjacent subjects of climatology, hydrology, and agricultural science.

Eq. (16.1) requires modification when momentum is the property under conside tion, for whereas it may be appropriate to neglect pressure and Coriolis forces in t differential problem of the first few metres, these forces must certainly be consider in the integral problem which is the maintenance, growth or decay of wind syste against surface friction. The appropriate equation is the vertical integral of t equation of horizontal motion which, neglecting the acceleration of the air – a reasona procedure if we seek only the mean condition – may be written

$$2\,\omega \sin\phi\, \mathbf{k} \times \int_0^\infty \rho\,(\mathbf{v} - \mathbf{J})\,dz = -\,\boldsymbol{\tau}_0$$

where **k** is a unit vertical vector, **v** is the actual and **J** the geostrophic vector wind, and other symbols are conventional. From observations made over land in mainly clear skies, and not therefore necessarily representative, it has been inferred that the mean wind so closely approximates the geostrophic at levels above about a kilometre or less that no contribution is made to the integral from higher levels. The bottom layer of air which on this classical view supports the earth's drag has come to be regarded as the terrestrial boundary layer; but Sheppard (1951b) has questioned the validity of this picture, particularly over the oceans. It is argued that westerly momentum is fed down the gradient through a turbulent troposphere from the near-tropopause level of maximum westerlies, right to the surface in the belt of surface westerlies but only to the level of maximum easterlies in the belt of trades, with upwards transfer of westerly momentum in lower trade levels. The very small surface cross-isobar flow in the westerlies and the large and height-sustained down-gradient motion in the trades then become qualitatively explicable. There is support for the tropospheric boundary-layer suggestion in the work of Riehl *et al.* (1951) on the NE. trades : see also White (1950). It calls for much further investigation, particularly as to the scale of the turbulence which may effect the transfer at different levels. Moreover, with the source and sink of westerly momentum (or angular momentum) at the surface in low and higher latitudes respectively, it is known that the poleward flux across the dividing latitude is concentrated around the tropopause. This loss of momentum from the upper levels of low latitudes must be made good either by a mean meridional circulation or else by *upward* transfer against the gradient – possibly selective in time and place, as discussed in other instances in Section 10. It is evident that the problems of the general circulation inevitably involve an interplay between turbulence on the smallest and grandest scales.

One of the barriers at present to a quantitative understanding of the mechanism of the general circulation is ignorance as to the absolute values of the source and sink of atmospheric angular momentum in tropical and temperate latitudes respectively. Were this knowledge available, the large-scale flux between latitudes necessary to sustain the circulation at different seasons could be inferred. An estimate has been prepared by Priestley (1951) based on ocean data, and so subject to the uncertainty of the wind-stress relation over the sea. Since the climatological stresses depend principally on the occasions of high wind, in which condition the wind-stress relation at sea appears to be best known (Montgomery 1951), the accuracy would be quite adequate for the application at its present stage. A more serious gap is the lack of estimates of comparable accuracy over land, which are more elusive owing to the greater variability of land roughness and, perhaps even more, to the circumstance that land-wind statistics are not available over all representative types of terrain.

V. Diffusion as a study in structure

17. *Particle motion a direct expression of turbulence*

One method of handling the problem of diffusion from point, line and area sources has been discussed in the previous chapter; here we consider another approach which, by fastening attention on the motion of individual particles of air, in a statistical sense, relates the structure of the turbulence to the diffusion of particles in a more direct way.

The classical work in this study is due to Taylor (1922) and Richardson (192 and is particularly relevant at present on account of the recent development of statistic theory generally. Taylor's work is based on the premise that the field of turbulen is homogeneous and steady, e.g., $\overline{u'^2}$ constant in space and time (above the minimu limits of space and time for which the mean is defined). It is therefore not strict applicable to the boundary layer and its application to the atmosphere must be ma with care since larger and larger eddies are taken into the mean ($\overline{u'^2}$ increasing) as t space or time intervals on which it is based are extended. This outstanding featu of atmospheric turbulence was in fact the main concern in Richardson's paper thou it went largely unheeded for many years, owing perhaps to the general predilection theoretical workers to study the steady state.

18. *Taylor's analysis; the Lagrangian correlation*

Taylor arrived at a quite remarkable result from very simple reasoning. Havi shown that differentiation and integration is possible within a meaned product, e.g that the condition that $\overline{u'^2}$ shall be constant is $\overline{u' \frac{\partial u'}{\partial t}} = 0$, it follows that the mea square deviation $\overline{X^2}$ of particles from their centre of gravity in the direction of a eddy motion u' as a function of time t from release is given by

$$\frac{d\overline{X^2}}{dt} = 2\,\overline{u'X} = 2\,\overline{u'^2}\int_0^t R_\xi\, d\xi \qquad . \qquad . \qquad . \qquad (18.$$

where R_ξ is the correlation coefficient between the eddy velocities $u'(t)$, $u'(t+\xi)$ *which the particle is subjected* at instants ξ apart in time. It is thus a Lagrangia coefficient of correlation for the component of turbulent velocity considered. N physical assumption regarding the structure of turbulence other than the constan of $\overline{u'^2}$ is involved in this expression. $\overline{X^2}$ is of course the measure of the diffusion particles in the given direction and may be readily related to the diffusion of densi or of heat, e.g., the local concentration of particles is analogous to the temperatu excess of a volume element in relation to the space in which the diffusion occu molecular diffusion apart. However it should be remarked that Eq. (18.1) is strict derived only for a group of particles whose initial motion is uncorrelated since t $\overline{u'^2}$ of the particles is identified, for any value of t, with the $\overline{u'^2}$ of the whole motio The result would therefore tend to overestimate the diffusion in the early stages fro a localized source which emits for only a short time.

The form of the R_ξ, ξ curve is from the present point of view the fundamen problem of turbulence. Only a dynamical theory of the motion can provide it, b we know that there must be a period near $\xi = 0$ in which R_ξ is practically unity an for large ξ it will be zero, having earlier acquired minima or negative values if the are any marked regularities in the eddying motion. Thus for small t ($R_\xi \simeq 1$ Eq. (18.1) gives

$$\overline{X^2} = \overline{u'^2}\, t^2 \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (18.$$

while for large $t\,(> t_1)$ and $R_\xi = 0$

$$\overline{X^2} = 2\,T\,\overline{u'^2}\, t + \text{const.} \qquad . \qquad . \qquad . \qquad . \qquad (18.$$

where $T = \int_0^{t_1} R_\xi\, d\xi$. Here T is evidently a mean 'life' of the eddies. We no

at Eq. (18.4), apart from the constant deriving from diffusion for $t < t_1$, is of the ickian form with diffusivity $K = T\overline{u'^2}$. This must be so since, for $\xi > t_1$, $u'(t)$ and $(t + \xi)$ are uncorrelated. While Eq. (18.3) might be expected to apply to atmos-heric diffusion, even in the boundary layer, the upper limit of t being found from bservation, it is less likely that Eq. (18.4) is applicable because Taylor's fundamental ssumption is not often well realized. This does not necessarily minimise the importance R_ξ to atmospheric diffusion but the coefficient is difficult to measure directly, since is difficult to track an element of air over any distance. Measurements are however ow being attempted (Edinger 1951) for small ξ.

Batchelor (1949a) has extended Taylor's original analysis and is able to demonstrate principle the part played by the different-sized eddies in the diffusion process. He nds that the weights attached to different frequencies (see Section 21) in dispersion e equal to $2(1 - \cos nt)/n^2$ times their corresponding weights in the energy or ress. For example, if $F(n)\,dn$ is the fraction of $\overline{u'^2}$ contained within frequencies n nd $(n + dn)$ then $\overline{X^2} = 2\int_0^\infty \overline{u'^2}\,F(n)\,\frac{1 - \cos nt}{n^2}\,dn$. For small t the weighting factor nds to t^2, independent of n, so that the frequencies contribute to the dispersion in roportion to their energies, but for large t the large eddies become dominant. Batchelor lso shows that if the distribution of X is normal the diffusion obeys Fick's law, that ux is proportional to gradient, but the coefficient of transfer is time dependent and $= \int_0^t u'(t)\,u'(t + \xi)\,d\xi = \tfrac{1}{2}\frac{d\overline{X^2}}{dt}$.

A study by Brier (1950) allows for the initial separation of the diffusing particles nd shows that a more general expression than Eq. (18.1) should include the correlation R between velocities of the particles at the initial instant of time. Some emphasis is laced on the difference between the instantaneous deviation of particles about their entre of gravity and the deviation in the position of the centre of gravity in a number repeated trials. Taylor's formulation would, in fact, apply to the sum of the two. In single trial the coefficient ${}_0R$ becomes significant. It is also found to be significant the relation of the Lagrangian correlation to the Eulerian (spatial) correlation between ie simultaneous motion of separate particles at later times. This relation has also een discussed by Inoue (1950, 1951).

19. *Diffusion in relation to scale*

Brier has taken a step towards reconciling Taylor's approach with that of Richard-n (1926) who introduced a treatment of diffusion based on the mean square *separation* particles. As already suggested, the standard deviation of particles from a continuous oint source at a given time from release (given time of diffusion) increases more or ss indefinitely with the duration of sample, as the particles become subject to larger d larger eddies. On the other hand the root-mean-square separation of particles a given time from release will not be subject to this systematic change as one sample ceeds another, for if the separation be small compared with the size of the large ldies, all particles will be displaced a roughly equal amount by the latter.

Instead of the one-dimensional Fickian equation of diffusion (cf., Eq. (15.1)) in hich the coefficient of diffusion K must be made a function $f(x)$ of x even if the rbulence is homogeneous, Richardson obtained a new one-dimensional equation of ffusion

$$\frac{\partial q}{\partial t} = \frac{\partial}{\partial l}\left(F(l)\frac{\partial q}{\partial l}\right)$$

Here q is the probability of finding particles with separation l at time t, and $F(l)$ i new coefficient of diffusion which logically increases with l. The problem is now determine $F(l)$ and again, in the absence of a description of the turbulent field, must resort to observations of q in cases for which the new equation may be solv Unfortunately observations of diffusion have until very recently provided no basis obtaining q and Richardson was forced to fit his new equation into the framew of the old. Thus in the special case of $f(x) =$ constant, $F(l) = 2f(x)$ – the facto appears because q involves every particle twice. Then from observations of diffus over a very wide range of scale ($l \simeq 10^{-1}$ cm to 10^8 cm) Richardson found $K = 0{\cdot}2$ or $F(l) = 0{\cdot}4\, l^{4/3}$

Something has here been gained in the physical description of a process l there is a necessary loss, for concentration χ is the quantity of practical concern and is not in general recoverable from q and l; q represents a special kind of meaning proc for χ, which is irreversible. Richardson showed however that the standard deviat of the q, l curve from $l = 0$ is $\sqrt{2}$ times the standard deviation of χ from its cen of gravity. We shall find the expression $F(l) \propto l^{4/3}$ arising in more recent treatme discussed in Chapter VI.

20. *Detailed application*

The work described so far does not go beyond a formulation of some aspe of the problem. In order to apply it quantitatively to atmospheric diffusion one m suppose the significant correlation functions to be known, either from direct measu ment or from some body of basic dynamical theory.

Lacking this knowledge at present we can only proceed by means of hypothet forms for the correlations. Sutton (1932) assumed

$$R_\xi = \left(\frac{a}{\bar{u}\,\xi}\right)^n \ldots . \quad 0 < n < 1$$

giving, from Eq. (18.1),

$$\overline{X^2} = \tfrac{1}{2}\, c^2\, (\bar{u}\, t)^{2-n}$$

where a is a constant length and n a dimensionless parameter of turbulence, both be determined by observation, and $c^2 = \dfrac{2\pi\, a^n}{(1-n)\,(2-n)}$ is a 'coefficient' of diffusi the eddy velocities having a Maxwellian distribution. In this formulation $\int_0^t R_\xi$ increases indefinitely with t, a feature which purported to represent the existence eddies with no upper limit of size. However, the recent spectrum analysis of Batche (1949a) has shown that the existence of vanishingly small frequencies is in no way variance with a law of the form (18.4).

Sutton (1947) has also used a form of R_ξ which satisfies the condition $R_\xi \to 1$ $\xi \to 0$ in Eq. (18.1) to provide explicit expressions for the concentrations from pu and from point and line sources. These are the only detailed solutions known to reviewers. In view of the hypothesis and lack of rigour in the application or, rath because any present treatment must suffer from these objections, the value of s

solutions must be judged by their accuracy in the field. This has been shown to be good for distances from 50 to a few thousands of yards under a limited range of conditions, but here again one must note the danger of extrapolation to scales and conditions, particularly to degrees of stability, other than those for which the accuracy has been experimentally established.

Since basic knowledge of structure is inadequate to predict atmospheric diffusion in detail, the inverse approach may be worth while. We note from Eq. (18.1),

$$R_\xi = \frac{1}{2\,\overline{u'^2}} \frac{d^2\overline{X^2}}{dt^2}$$

Thus an experimental study of diffusion is capable of leading back to the fundamental parameter of homogeneous turbulence. The method has yet to be exploited.

VI. Current studies in structure

21. *Spatial and temporal correlation and the energy spectrum*

In the previous section we were concerned only with kinematical and statistical properties of a turbulent field; no reference was made to the physical properties of the fluid or to the forces in operation. In this final section we shall try to indicate the main trends of modern theory, in which the approach is again largely statistical but some attempt is made to introduce more physical ideas. The reader will judge for himself what success or at least what promise these methods provide. The complexity of the problem is certainly great so that striking advance is hardly to be expected.

Modern theories are largely concerned with Eulerian correlations between various properties of the turbulent field in space or at a fixed point in time. Examples of the former are the correlations r_x, r_y, r_z between, say, one component of the turbulent velocity at a given point and the same component at varying distances x, y, z respectively from the given point as origin. An example of the second class is the auto-correlation r_ξ between fluctuations of a chosen quantity at a fixed point at varying intervals ξ in time. Such Eulerian functions are in general more readily measured than the Lagrangian function R_ξ of Chapter V since the apparatus involved is fixed relative to the boundaries.

The auto-correlation function r_ξ for a given component velocity determines the energy spectrum of that component, regarded as a Fourier synthesis,* and vice versa. Thus if $F(n)\,dn$ be the fraction of $\overline{u'^2}$ contained in a frequency band n to $(n + dn)$

$$F(n) = 4\int_0^\infty r_\xi \cos 2\pi n\xi \, d\xi \ldots . \int_0^\infty F(n)\,dn = 1 \quad . \quad . \quad (21.1)$$

with a corresponding relation for r_ξ as a function of $F(n)$ – they are Fourier cosine transforms of each other, multiplicative constants apart. A similar relation is approximately true for the spatial correlation functions, provided the fluctuations are small compared with the mean flow (Taylor 1938), e.g.,

$$F(n) = \frac{4}{\bar{u}}\int_0^\infty r_x \cos\frac{2\pi n x}{\bar{u}}\,dx \quad . \quad . \quad . \quad . \quad (21.2)$$

In the laboratory, where the spectrum may be made to contain a negligible fraction

* Each component of velocity at a point is a function of time which has the character of periodicity without a fundamental period. This distinguishes turbulence from sound and from the precursors of turbulence such as Benard cells. Thus the mathematical representation of a turbulent motion is not given by the ordinary Fourier series, but by the 'almost periodic' series or Fourier integral, i.e., $u'(t) = \int_{-\infty}^{\infty} A(n) \exp(2\pi i n t)\,dn$ where $A(-n) = A^*(n)$, A^* being the complex conjugate of A.

of energy in frequencies below several cycles per second, both $F(n)$ and r_x can b directly measured by modern electronic and other techniques and there is an accumu lating body of knowledge of these quantities and their relations to other properti of the field in unsheared flow (see e.g., Goldstein (1938), Dryden (1943), and Agostin and Bass (1950) for summaries, and Stewart and Townsend (1951) for more recen work), and in sheared flow (Laufer 1950). Evaluation in the atmosphere is less simp on account of the more or less indefinite extension of the spectrum into larger an larger eddies, but some information is given by Giblett *et al.* (1932), Shiotani (195 and R. J. Taylor (1951) in the lower atmosphere over the land, and by Sheppar (1951a) over land and sea.

22. *Energy decay in isotropic turbulence; microscale of turbulence*

Small eddies are associated with large velocity gradients and so are responsibl for the main part of the viscous dissipation, and it is therefore evident that the dissipatio must be closely related with the form of the spatial correlation at small values of th arguments. Taylor's (1935) analysis of this relation refers to *isotropic* turbulence fo which the average value of any function of the velocity components or their spati derivatives is unaltered by rotation or reflection of the axes of reference. Here the $\overline{u'^2} = \overline{v'^2} = \overline{w'^2}$ and $\overline{u'w'}$, etc., $= 0$, so that boundary-layer turbulence is strictl excluded. There is reason however to believe that even here the eddies in the dissipatio range are isotropic although the bulk of the turbulence is not.

From the Navier-Stokes equations, the mean rate of dissipation ϵ of isotropi turbulent energy per unit mass is

$$\epsilon = 7{\cdot}5\,\nu\,\overline{\left(\frac{\partial u'}{\partial x}\right)^2} \qquad . \quad . \quad . \quad . \quad . \quad (22.1$$

Since, as shown by Taylor (1922),

$$r_x = 1 - \frac{1}{2!}\frac{x^2}{\overline{u'^2}}\overline{\left(\frac{\partial u'}{\partial x}\right)^2} + \ldots .$$

it follows that

$$\epsilon = 15\,\nu\,\overline{u'^2}\,\operatorname*{Lt}_{x\to 0}\left(\frac{1 - r_x}{x^2}\right) \qquad . \quad . \quad . \quad (22.2$$

The limit on the right is given by the curvature of the r_x, x curve at $x = 0$. Measure ments on decaying turbulence behind a grid in a wind-tunnel – the rate of deca downstream gives ϵ – are in excellent conformity with this relation. Taylor (193 also defined a length λ, given by

$$\frac{1}{\lambda^2} = \operatorname*{Lt}_{x\to 0}\left(\frac{1 - r_x}{x^2}\right) \qquad . \quad . \quad . \quad . \quad (22.3$$

so that

$$\epsilon = 15\,\nu\,\overline{u'^2}/\lambda^2 \qquad . \quad . \quad . \quad . \quad . \quad (22.4$$

The quantity λ has been called the *microscale* of turbulence as it is a measure of th small eddies which are responsible for the viscous dissipation. Taylor deduced th following equation for λ from dimensional considerations

$$\lambda = A\,(L\,\nu/\sqrt{E'})^{1/2}$$

where L is a linear dimension of the turbulence-producing device, e.g., mesh-siz

of grid, or, in uniform flow, of the largest eddy, E' is the turbulent energy per unit mass, and A a numerical constant of order unity. This formula also was found to be consistent with the decay of turbulence in a wind tunnel. Observe that as the turbulent energy increases, the microscale decreases – smaller eddies may be maintained against viscosity when larger turbulent energy is available to feed them. Typical values of λ in wind-tunnel turbulence are a few mm; in the atmosphere near the ground R. J. Taylor (1952) finds values from 2 cm at a height of 2 m to 13 cm at 30 m and we may expect it to be much larger in the free atmosphere where L probably increases without corresponding increase in E'.

23. *The similarity hypotheses*

The concept of an eddy spectrum and of the related correlation function discussed in Section 21 above throws up the possibility of a quantitative analysis of the turbulence in different parts of the spectrum selected according to the major physical processes which may be operative in each part. The analysis of the previous section is a particular instance and current work which we shall now shortly describe is a broadening of that approach. It stems from a rather remarkable series of papers put forward independently during and immediately following World War II by Kolmogoroff, Onsager, v. Weizsäcker, and Heisenberg, the approach by the first author being through the correlation function and by the others through the energy spectrum.

The field of turbulence considered is essentially one in which energy ' enters ' the spectrum in the long-wave (low frequency) range from the mean motion and ' leaves' it at the short-wave end by viscous dissipation into heat, transformations of scale in the intervening wavelengths being determined by the dynamical properties of the fluid and the turbulent energy. Thus at any intermediate wavelength energy is on the whole generated by the breakdown of larger eddies and simultaneously dissipated by breakdown into smaller eddies. Viscous dissipation will in fact occur at all wavelengths but it is only in the small eddies that the velocity gradients are sufficiently large to give rise to appreciable viscous dissipation. Moreover a point is probably reached in the short-wave end where, for a given amount of turbulent energy, dissipation by viscosity is so great that few if any smaller eddies are formed. In a steady state of motion there will then be an equilibrium energy spectrum whose form will depend on the ' capacity ' for energy transfer of the various sizes of eddy present in the spectrum. And, except towards the ends of the spectrum where energy is either building up from mean motion (in large eddies) or decaying partly or even largely through viscosity (in small eddies), the amount of energy passing from one wavelength to the next will be independent of wavelength and equal to the *total* viscous dissipation. The processes involved are shown diagrammatically in Fig. 5. No special significance attaches to the exact positioning of the maximum in the figure.

Before proceeding we may ask how well such ideas conform with what we know of the *atmospheric* spectrum. This is characteristically *unsteady*, and it is evidently desirable that we should know how rapidly a change in one part of the spectrum will produce changes in the other parts. There is much scope here for dynamic research, but the complications due to radiation must be recognized, while energy may enter or leave the spectrum in any sub-range where buoyancy affects the motion. We need to discover below what period the form of the energy spectrum is uninfluenced by buoyancy, and to study the spectrum of associated quantities such as the heat flux.

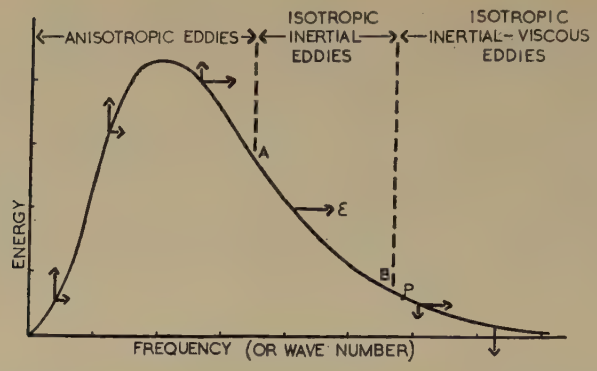


Figure 5. Diagrammatic representation of energy transfer processes in a steady turbulence spectrum in which similarity in eddy structure is assumed to exist among eddies smaller than those represented by the point A and viscous dissipation is appreciable only in eddies smaller than B.

↑ = energy input from mean motion (total = ϵ)
→ = energy transfer from larger to smaller eddies
↓ = energy dissipated by viscosity (total = ϵ)

The possibility also arises that ' energy traffic ' through the spectrum need not necessarily be always or primarily one-way. Cumulus convection may be one case of a larger system aloft from smaller systems near the surface; less directly but more strikingly, the latter in their ability to transfer heat and water vapour from the surface provide the origin of much of the energy of the largest atmospheric motions. Are there then some forms of long-wave spectra particularly favourable to the organization of energy from the small to the larger systems despite an ever-present degradation down the scale ? The question leads back into the difficulty that, in the absence of any clear-cut distinction between mean and turbulent motion, the form of the long-period end of the spectrum will depend on the length of record analysed.

We should not then be too ready to identify the atmospheric spectrum with the quasi-steady ' cascade ' spectrum which is the basis of much recent work. But it is probable that there is a sufficiently wide range of wavelengths and sufficiently steady conditions sometimes to be encountered in the atmosphere to make the findings of Kolmogoroff and others of interest. So we now come to the simplifying hypotheses by which Kolmogoroff sought to establish the laws of some parts of the spectrum. The large-scale eddies of a system are generally anisotropic; they are characterized by more or less recognizable structure or pattern, e.g., the cyclone, the cumulus, or the larger, stress-producing elements in boundary-layer turbulence. As the energy is transformed into smaller-scale systems there is an increasing tendency towards equi-partition of energy among the components of the motion and an increasing lack of correlation between them. Thus as the eddy scale diminishes the eddies show fewer and ultimately no marks of their progenitors, other than the energy derived from them. Beyond this point in the spectrum similarity obtains. The energy in a particular wave length will also depend on the rate at which it can be passed into still smaller wave lengths and into heat by viscous dissipation, the spectral form in the similarity range being thus determined. If the spectrum is wide, if, that is, the Reynolds number of turbulence $LE'^{1/2}/\nu$ is large (here L is a representative dimension of the large eddies which contain a substantial fraction of the total turbulent energy E') there will exist a range of eddies which are both isotropic and independent of the fluid viscosity. This

ange, flanked on the one side by anisotropic eddies and on the other by viscosity-ontrolled eddies, is termed the inertial sub-range (see Fig. 5). Thus Kolmogoroff as led to make the two following similarity hypotheses :

(1) The average properties of the smaller-scale components of any turbulent motion are determined by ϵ and ν only,

(2) Within the range of smaller-scale components described by (1) there is an upper (inertial) sub-range whose average properties are determined by ϵ only.

Notice that the hypotheses refer to *any* turbulent motion and, in particular, do not emand that the turbulence as a whole should be steady. Kolmogoroff in fact makes se of the concepts of *local homogeneity* and *local isotropy*, the field being locally omogeneous if the statistical properties considered depend, within the spatio-temporal omain of homogeneity, only upon the spatial *separations* and time *differences* involved n their formulation. Local isotropy obtains when there is local homogeneity and the tatistical properties are independent of the orientation and sense of axes. Thus the omain to which the hypotheses refer must be small compared with the large eddies nd with the time in which an appreciable change of energy of the large eddies may ccur. The hypotheses say nothing directly about the position in the spectrum at hich they become valid. That will depend on the particular field, and will either e inferred from quite rough arguments or else by appeal to observation.

24. *Predictions from the similarity hypotheses*

Dimensional analysis may now be used to establish relations between those tatistical properties of the turbulence which depend on the smaller eddies, the quantities and ν, and any length or time element involved in the properties considered, while a ynamical relation for ϵ is available from Eq. (22.2) or Eq. (22.4). The hypotheses lo not themselves enable one to select the statistical properties which derive from the maller eddies; this selection is usually made on rather intuitive grounds. However, f the analysis requires a length or time to be formed from ϵ and ν, the required length s $(\nu^3/\epsilon)^{1/4}$ and the required time $(\nu/\epsilon)^{1/2}$. These evidently provide measures of eddy ize or period in which viscosity exerts marked control and so provide units against hich the spectrum may be gauged. (The length $(\nu^3/\epsilon)^{1/4}$ is much smaller than the icroscale λ). We now reproduce a few of the deductions which have been made cf., Batchelor 1947, 1948).

(i) One-point correlations, e.g., mean square of fluctuating pressure gradient. Here, from the first hypothesis and Eq. (22.4)

$$\overline{\left(\frac{1}{\rho}\frac{\partial p'}{\partial x}\right)^2} \propto \left(\frac{\epsilon^3}{\nu}\right)^{1/2} \propto \frac{\nu E'^{3/2}}{\lambda^3} \quad . \quad . \quad . \quad (24.1)$$

Heisenberg (1948a) showed that the mean-square pressure gradient should depend n the smaller eddies while $\overline{p'^2}$, like $\overline{u'^2}$, is largely determined by the big eddies).

(ii) Two-point correlations, e.g., r_x for u'. Here a spatial separation l (in the -direction) is involved whence we derive, for values of l less than the size of the mallest eddies with directional properties

$$\overline{[u'(x) - u'(x+l)]^2} = 2\,\overline{u'^2}\,(1 - r_l) = (\nu\epsilon)^{1/2} f\left\{\frac{l}{(\nu^3/\epsilon)^{1/4}}\right\} \quad . \quad (24.2)$$

or the inertial sub-range ν must disappear from the relation, whence

$$2\,\overline{u'^2}\,(1 - r_l) = C\,(l\epsilon)^{2/3} \quad . \quad . \quad . \quad (24.3)$$

where C is a constant whose value has been derived theoretically by Obukhov a Yaglom (1951) as 1·59 or 2·12 for longitudinal or transverse correlations respectively decaying turbulence.

(iii) Lagrangian time-correlations, e.g., R_ξ for u'. Here a time ξ is involv and, for intervals less than the period of any eddies with directional properties,

$$2\overline{u'^2}(1 - R_\xi) = (\nu\epsilon)^{1/2} g\left\{\frac{\xi}{(\nu/\epsilon)^{1/2}}\right\} \quad . \quad . \quad . \quad . \quad (24$$

and for the inertial sub-range,

$$2\overline{u'^2}(1 - R_\xi) = D\,\xi\,\epsilon \quad . \quad . \quad . \quad . \quad . \quad (24$$

where D is another constant. Note that the correlation functions here and in derive from velocity differences which provide the means of separating large edd from small.

(iv) Eulerian time-correlations, e.g., r_ξ for u'. In this case some care must taken in identifying the basic time element involved. In the wind tunnel it is thou valid to assume a constant pattern of turbulence swept forward at the mean speed so that ξ is in effect replaced by a basic space element $\bar{u}\xi$ which for the inertial sub-ran would lead to (cf., Eq. 24.3)

$$2\overline{u'^2}(1 - r_\xi) \propto (\xi\,\epsilon)^{2/3} \quad . \quad . \quad . \quad . \quad . \quad (24$$

But in the atmosphere this assumption is more doubtful; to the extent that it is va to recognize ξ as the basic time element in its own right, the similarity relation wou be (cf., Eq. 24.5)

$$2\overline{u'^2}(1 - r_\xi) \propto \xi\epsilon \quad . \quad . \quad . \quad . \quad . \quad (24$$

(v) By similar methods the form of the energy spectrum in the inertial sub-ran can be derived as

$$\overline{u'^2}\,F(n) \propto \epsilon\, n^{-2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (24$$

or, as it is more usually expressed, in terms of wave number k (k = number of wav per unit distance),

$$\overline{u'^2}\,F(k) \propto \epsilon^{2/3} k^{-5/3} \quad . \quad . \quad . \quad . \quad . \quad (24$$

25. *Tests of the similarity hypotheses*

Stewart and Townsend (1951) have recently found that, for the Reynolds numbe of turbulence (*c.* 1000) attainable by them in a wind tunnel similarity only obtai for $k > 0{\cdot}6\,(\epsilon/\nu^3)^{1/4}$, that is well within the viscous dissipation range. They inf moreover that an inertial sub-range ($k < 0{\cdot}1\,(\epsilon/\nu^3)^{1/4}$) will exist only when the Reynol number for the viscosity-controlled eddies, $\lambda E'^{1/2}/\nu > 1000$. This condition likely to be satisfied in most cases of atmospheric turbulence (cf., Section 22) so th both Kolmogoroff hypotheses may be inferred to have meteorological applicatio R. J. Taylor (1951) has in fact shown that, at heights from 2 m to 30 m, a linear relati between r_ξ and ξ (Eq. (24.7)) obtains over a range of ξ from 2 sec, and probably le up to $\xi = z/\bar{u}$, that is up to eddy sizes approximately equal to the height above ground.

Batchelor (1950) has discussed those consequences of similarity which mig apply to the atmosphere, that is such processes as do not depend on the larger, anisotrop eddies. He finds these processes to be of the diffusing cluster* class because the spre

* It was shown in Section 18 that diffusion *from a fixed point* is at no time dominated by the smaller eddies and so can be treated by similarity methods.

is here dependent on velocity *differences*. He deduces expressions for the rate of change of mean-square separation of particles, the problem which Richardson had earlier discussed (cf., Section 19), but now with limitations on initial separation and time of diffusion in order to retain validity for the similarity hypotheses. Suppose a large number of pairs of particles with initial separation l_0 to be released, then the mean-square separation of the pairs $\overline{l^2}$ at time t after release, always in the inertial sub-range, is given by dimensional analysis in terms of l_0, t and ϵ

$$\frac{\overline{l^2} - l_0^2}{l_0^2} = \frac{4}{3} c_1 \left(\frac{t\,\epsilon^{1/3}}{l_0^{2/3}}\right)^2 \quad \ldots\ldots t \ll l_0^{2/3}/\epsilon^{1/3} \text{ or } (\overline{l^2} - l_0^2) \ll l_0^2 \quad . \quad (25.1)$$

and

$$= c_2 \left(\frac{t\,\epsilon^{1/3}}{l_0^{2/3}}\right)^3 \quad \ldots\ldots t \gg l_0^{2/3}/\epsilon^{1/3} \text{ or } (\overline{l^2} - l_0^2) \gg l_0^2 \quad . \quad (25.2)$$

where c_1 and c_2 are constants of order unity.

Charnock (1951b) has tested Eq. (25.1) by means of the scatter of smoke puffs in the free air reported by Durst (1948), for l_0 between 40 m and 800 m and t between 10 sec and 50 sec. The analysis was inconclusive but there was some evidence for the validity of Eq. (25.1) in winds less than 5 m/sec for which ϵ fell between 0·2 and 0·9 erg g^{-1} sec^{-1}. This compares with a *mean* value of ϵ computed otherwise by Brunt (*Physical and dynamical meteorology*, 1939) of 5 erg g^{-1} sec^{-1} for the free atmosphere. The minimum value of $l_0^{2/3}/\epsilon^{1/3}$ in Charnock's analysis was 800 sec, that is well above the values of t used in the analysis.

Eq. (25.2), for large t, may be written in differential form,

$$\frac{d\overline{l^2}}{dt} = 3\,(c_2\,\epsilon)^{1/3}\,\overline{l^2}^{2/3}$$

If this be treated as the Fickian expression for the spread of particles about their mean position, then the corresponding coefficient of diffusion $K\,(= \frac{1}{2}\frac{d\overline{l^2}}{dt}$, cf., Section 18) is

$$K \propto \epsilon^{1/3}\, l^{4/3}$$

where l is the scale of the diffusion process. This 4/3-power relation is that derived from Heisenberg's treatment of eddy viscosity (see below), and as found by Richardson (cf., Section 19) over a very wide range of l, much wider in fact than would be expected from the similarity hypotheses and our present ideas about the isotropy of atmospheric turbulence. Moreover the numerical coefficients, with $\epsilon \simeq 1$ erg g^{-1} sec^{-1}, are about the same in the two cases. These agreements must be regarded as somewhat fortuitous and hardly as evidence, as some have supposed, of the validity of the second similarity hypothesis over the wide range of l to which Richardson's relation roughly applies. It may be, however, that very large atmospheric eddies, which certainly are not isotropic (their vertical motions are necessarily small compared with their horizontal motions) tend to a state of restricted isotropy, in the horizontal. This is indeed shown to be the case by the work of Brooks *et al.* (1950), on the turbulence of the general circulation, although sufficient anisotropy must remain to provide a sensible meridional flux of momentum as evaluated by Priestley, Widger and others.

26. *Weizsäcker-Heisenberg theory of energy transfer*

Weizsäcker (1948) dealt directly with energy-transfer processes instead of with correlation functions, and his treatment has been elaborated by Heisenberg (1948a, b).

He considers the energy ϵ_P lost by eddies larger than some given size, $k < k_P$, that the energy lost from that part of the spectrum to the left of some point P in Fig. 5. Some of ϵ_P is in general due to viscous dissipation within the wave-number range to k_P itself and the remainder is used to maintain smaller eddies ($k > k_P$) against their own viscous dissipation. Now the viscous dissipation is realized through a coefficient of molecular transfer, the molecular viscosity ν, and Weizsäcker assumes that the transfer of energy from larger to smaller eddies at $k = k_P$ is also realized through a coefficient of transfer, an eddy viscosity K_P. He then determines the form of K_P on the basis of similarity in eddy structure.

The total viscous dissipation ϵ, Eq. (22.1), may be written in spectral terms (Heisenberg 1948a, b)

$$\epsilon = 2\,\overline{u'^2}\,\nu \int_0^\infty k^2\,F(k)\,dk$$

which form, incidentally, brings out the importance of small eddies (large k) in viscous dissipation, through the presence of k^2 in the integral. The rate of viscous dissipation in wave numbers less than k_P is then $2\,\overline{u'^2}\,\nu \int_0^{k_P} k^2\,F(k)\,dk$ and, using the concept of eddy viscosity for the energy transfer from large to small eddies, we have

$$\epsilon_P = 2\,\overline{u'^2}\,(\nu + K_P) \int_0^{k_P} k^2\,F(k)\,dk \qquad . \qquad . \qquad . \qquad (26.1)$$

Now $\underset{k_P \to \infty}{\text{Lt}}\ K_P = 0$ so that K_P may be represented by an integral $\int_{k_P}^\infty$. In the range of similarity the function under the integral will not depend on k_P, but is assumed by Heisenberg to depend on k, both directly and through $\overline{u'^2}\,F(k)$. Dimensional analysis then shows that

$$K_P = \text{const.}\ \overline{u'^2}^{1/2} \int_{k_P}^\infty \left(\frac{F(k)}{k^3}\right)^{1/2} dk \qquad . \qquad . \qquad . \qquad (26.2)$$

In the inertial sub-range, Eq. (24.9), $F(k) \propto \epsilon^{2/3} k^{-5/3}$ from which, with the last equation

$$K_P \propto \epsilon^{1/3} k_P^{-4/3} \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (26.3)$$

that is, increasing as the 4/3 power of the size of the largest eddy among those contributing to K. In earlier parlance, the integral in Eq. (26.2) could be called the mixing length, no longer a constant in a given position, but increasing with increase in scale of the transfer process concerned. The expression (26.3) for K_P could have been written down directly from dimensional considerations, but the above treatment has the merit of linking K_P with the energy spectrum and of showing its dependence on the larger eddies through the $k^{-3/2}$ factor in the integrand of (26.2).

Heisenberg uses the hydrodynamical equations of motion for isotropic turbulence in combination with Eqs. (26.1) and (26.2) to derive the form of the spectral function throughout the similarity range, making assumptions for which we must refer the reader to the original papers or to Agostini and Bass. For small values of k he obtains the $-5/3$ law as an approximation while in large values of k, $F(k) \propto k^{-7}$.

Batchelor (1949b) treats homogeneous turbulence in an infinite domain and shows that the expression (26.1) is not only physically reasonable but is analytically exact for energy transfer from the biggest, anisotropic eddies. He considers a generalized form of (22.2) but now in terms of a vector wave number $\mathbf{k}$ (at the sacrifice of some

ecision we will adhere to the one-dimensional functions). Existence of the transform quires the integral of r over all space to converge, i.e., the correlation at large distances ust fall off more rapidly than the cube of the distance (which may conflict with ality in the atmosphere). It then follows, solely from continuity, that $F(k) \propto k^4$ for all k, or that the energy in numbers less than k varies as k^5.

The complete Navier-Stokes equations are transformed in a similar manner, and pansion in ascending powers of k then displays the behaviour of the larger eddies – e complementary approach to that of Section 22. In general it is shown that three ocesses are in operation at any point in the spectrum : viscous dissipation, a transfer energy from one component velocity to another tending to equipartition of energy – a sult of the operation of the pressure fluctuations – and a transfer of energy between fferent wave numbers – an inertial effect. For small k all three processes are shown be small so that the largest eddies remain practically unchanged following formation. the turbulence generally is decaying then the big eddies ultimately become dominant.

The atmosphere is not an infinite simply-connected domain, and this combined ith its rotation must force a state of non-homogeneity, even in the horizontal, on e largest scale. Other limitations of the present treatments will occur to the meteoro-gical reader, but we would prefer to close on a hopeful note in remarking the progress ade towards our needs by the extension to anisotropy and to a turbulent field assumed exist without mean motion.

VII. Epilogue

The present stage is one of reorientation of view and of technique in viewing. We ave pleaded for greater effort on the class of ' integral ' problems, but this is only one spect of a broader need. For too long a time turbulence has remained on the fringe f meteorology, definitely earthbound, and concerned with the absolutely small (on e terrestrial scale). Any motion large enough to be visualized or represented synoptic-ly was in another category (and rightly so from certain points of view) so that idealized ntities called depressions, anticyclones, long waves, etc., dominated dynamical eteorology. The authors will not have failed essentially in their purpose if they ave indicated, however tentatively, the scope for taking all atmospheric motion into eir field, for getting rid of the easy acceptance of a mean motion in which ' turbulence ' s the embroidery of the anemogram) is securely embedded. It appears implicit in e conclusions of the previous review in this series (Sutcliffe 1952) that the turbulence pproach which we are glimpsing must be followed if longer-period changes in weather re to be treated quantitatively.

A hard task lies ahead, for the effective formulation of the extended domain waits et to appear, but there is already a developing technique of fluctuation study to our and. Its exploitation is required on all scales – we have instanced the need for studies aralleling those of Laufer on the smaller scale, and have made some mention of ecent work on the grandest scale. There is a wide territory between. Some meteoro-gists are nervous of the statistical approach for fear of ' losing the physics.' The anger is admitted but the statistical and the synoptic are not exclusive and each ould provide guides for thought to the other.

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The slopes of cumulus clouds in relation to external wind she

By JOANNE S. MALKUS
Woods Hole Oceanographic Institution and Illinois Institute of Technology

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SUMMARY

An equation for the slope of cumulus clouds is derived. It is shown to depend upon the ver ascent rate, the external wind shear and the horizontal frictional forces between the cloud and surroundings. These frictional forces are of two origins : one due to entrainment or mixing with outside air, and the other due to form or body drag.

Four observational cases are presented, one concerning trade cumulus, and three involving cl in mid-latitudes. In two of these, measurement of updraught speed, wind shear, and cloud slope pe an estimate of the magnitude of the friction forces. The minimum contribution due to entrainme approximately equal to that inferred by Stommel for trade cumulus.

1. INTRODUCTION

The effect of wind shear upon the dynamics of convection requires attention in degree that interaction between convective elements and the surrounding air recognized. In recent years sufficient observational evidence has been gathered shed doubt upon the adequacy of models based upon the non-interacting parcel. manner in which these observations depart from parcel-method predictions is in it suggestive of a direction in which modifications may be attempted. Lapse rates in convective clouds have been found to be *greater* than moist adiabatic and to fluctu irregularly with height; observed temperature excesses and updraught strengths h been smaller than if the cloud air followed the moist adiabat ascribed to it by pa considerations; cloud tops have *ceased* growing when vast amounts of ' positive ar were available; and liquid-water content has been often found to be only a fract of that expected.

The first important step toward a cloud model possessing these features was m by Stommel (1947). He showed that the lapse rates found in several examples trade cumuli could be accounted for if an amount of outside air were entrained int mixed with the rising cloud. Such entrainment would constitute an interaction betw cloud and surroundings by which some of the momentum, vapour content, and h of the environment is imparted to the cloud, thus creating a resistance to its grow Studies succeeding Stommel's have shown that if entrainment or mixing does occu the magnitude that he inferred, a consistent picture of cumulus convection is possi which possesses the observed departures from the parcel model.

Owing to the difficulties of measurements within clouds, however, many meteorc gists have been awaiting more definite evidence of the occurrence and magnitude cloud-environment interaction. The slope of a cumulus is recordable with the simp camera, and can be used in conjunction with wind profiles to supply an independ determination. Evaluation of the resistance forces on updraughts is therefore primary objective of this paper on cloud slopes. An additional and corollary object is to predict and verify a quantitative relation between cloud slant and wind sh and by means of the model so constructed to explain the well-known observation t under equal stability and moisture conditions a strongly slanting cloud is less lik to reach great heights than one which grows more nearly vertically.

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2. Theory

The slope of an updraught (whether there are condensed water droplets in it or not is, for the moment, unimportant) at any level depends only upon the ratio of its vertical ascent rate to the departure of its horizontal velocity at that level from that at its base, namely,

$$\tan\theta = \text{slope} = \frac{w}{u_c - u_{E_0}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the horizontal coordinate, x, is chosen in the plane in which the slope is to be determined and is taken positive in the direction of the component in this plane of the wind at draught base. The angle θ is measured in degrees counterclockwise from the positive x-axis. The vertical draught speed is w; the horizontal speed of the draught air in the x-direction is u_c, and its horizontal speed at its base is u_{E_0}, or the speed of the external wind.

(a) *Determination of the horizontal speed of the air in an updraught*

If a rising body has very great inertia relative to the forces exerted on it by the surrounding air, then it will retain most of its initial horizontal velocity, undergoing little deflection. The parcel method implies that convective elements belong to this category, and therefore that the slant of cumulus clouds should be negligible (i.e., $\theta \simeq 90°$). If, however, a rising body such as a pilot balloon has relatively small inertia its horizontal velocity will approximate that of the air through which it is passing, and the slant of its upward path will be a maximum. Observations discussed by Malkus (1949) and Byers and Braham (1949) show that the behaviour of a rising cumulus lies between these extremes.

The nature of the drag forces operating on cumulus has been described by Schmidt (1947). There are essentially two ways in which a rising air bubble or column exchanges horizontal momentum with its surroundings. The first is due to entrainment into the draught of a portion of the outside air, and the second, to the draught's pushing air aside and around itself, creating a ' countercurrent ' in the horizontal by means of a pressure difference across its boundaries. This latter force has been called ' form ' or ' body ' drag. In the original work predicting the magnitude of cloud-air velocities, entrainment was considered as the only frictional force, and it was assumed that all the entrained air was carried along with the updraught. However, clouds or thermals apparently maintain an approximately constant cross section with height and observations (especially time-lapse motion pictures, but also some airplane traverses through clouds reported on by Malkus 1949) suggest that some of the cloud air is detrained, so that the draught retains roughly only that fraction of the influx required to preserve continuity without significantly expanding its cross-sectional area (see discussions by Austin 1951, and Malkus 1952). The following derivation of an expression for the horizontal draught velocity is somewhat more realistic and general than the previous work in that it can include form drag and detrainment.

In the present model, as in those preceding it (Stommel 1947, Malkus 1949), an updraught in a steady state is considered. The rising air is assumed to be well mixed, homogeneous, and discontinuously distinct from its surroundings. A form of Newton's second law of motion is applied to a small vertical segment of the updraught between the levels z and $z + dz$. An x-z slice is schematically illustrated in Figure 1. At z the upward mass flux across the entire rising column is M g/sec; its speed in the

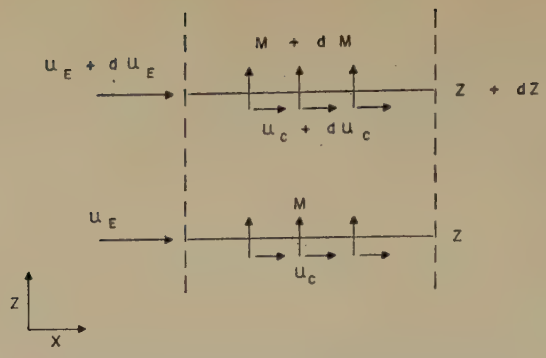

Figure 1. Schematic vertical cross section through a homogeneous updraught. M is the upward mass flux through level z; u_c is the x-component of the horizontal draught speed; u_E is the x-component of the external wind. At the level $z + dz$, these have become $M + dM$, $u_c + du_c$ and $u_E + du_E$, respectively. The boundaries of the draught are indicated by dotted lines since they in no way act as walls; nor need the cross-sectional area remain constant. The change in momentum flux between level z and $z + dz$ is considered in the theory.

x-direction is u_c cm/sec; and the x-component of the external wind is u_E cm/sec. At $z + dz$, the upward mass flux in the updraught has become $(M + dM)$ g/sec where dM is the net increase of flux brought about by entrainment. The speed of the draught in the x-direction is $(u_c + du_c)$ cm/sec; and the x-component of the external wind is $(u_E + du_E)$ cm/sec. The net increase in flux due to entrainment, dM, can be expressed as follows :

$$dM = dM_i - dM_0 \quad . \quad . \quad . \quad . \quad . \quad (2$$

where dM_i would be the increase in flux if all entrained air were retained and dM_0 is the flux of draught air lost by detrainment.

The momentum-flux equation for the segment is therefore

$$d(Mu_c) = u_E\, dM_i - u_c\, dM_0 + f dz \quad . \quad . \quad (3$$

where the left side of the equation represents the increment of horizontal momentum flux in the draught between z and $z + dz$, and the right side may be regarded as the sum of the forces acting. The first two terms on the right represent the changes of momentum flux due to entrainment, and the third term is the form-drag force where f represents the force per unit height upon the rising column.

If Eq. (3) is expanded, divided through by M and dz, and relation (2) is used, it becomes

$$\frac{du_c}{dz} + (u_c - u_E)\frac{1}{M}\frac{dM_i}{dz} = \frac{f}{M} \quad . \quad . \quad . \quad (4)$$

This equation for u_c as a function of z can be integrated if the functional dependence upon z of M_i, M, u_E and f is known.

In general, observations of the latter quantities are, at best, only available at distinct levels. At this stage, therefore, the simplicity of integrating the equation by steps is justified, assuming for the interval Δz from one measurement to the next that

$$u_E = u_{E_0} + bz \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{1}{M}\frac{dM_i}{dz} = \frac{1}{\overline{M}}\frac{dM_i}{dz} \equiv P = \text{const.} \quad . \quad . \quad . \quad (6)$$

and

$$\frac{f}{M} = \frac{f}{\overline{M}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where $\overline{M}$ is the average mass flux in the draught in the height interval Δz and P is the fraction of its average mass flux exchanged per unit height between draught and environment. It is clearly the gross entrainment, $\frac{1}{M}\frac{dM_i}{dz}$, or total amount of outside air mixed with the cloud, which is calculated by the Stommel method of comparison between cloud and environment soundings and his results indicate that P is of the order of magnitude of 10^{-5} cm^{-1}.

(*b*) *The form drag*

Rather little is known at present concerning the form drag on clouds. It seems probable that its horizontal component might resemble and possibly be of the same order of magnitude as the drag on a solid cylinder of comparable size moved at comparable speed through the air.

Aerodynamicists give the form drag force per unit height on the cylinder in air as

$$F = C_D \frac{\rho u^2 d}{2} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where ρ is the air density, d the cylinder diameter, u the velocity of the cylinder relative to the air, and C_D the dimensionless ' drag coefficient.' Experimentally, this coefficient is found to depend upon the Reynolds number, or the extent to which turbulence around the cylinder is developed. For Reynolds numbers in the range 10^5 to 10^6, C_D for solid cylinders drops abruptly from a fairly constant value of about 1·5 to about 0·3 at the point where the boundary layer becomes turbulent and thereby clings more closely, giving a more restricted wake. The Reynolds number for clouds is about 10^8 to 10^9, so that C_D might be expected to be about 0·3. However, what the behaviour of the boundary layer will be in the case of an air ' cylinder ' moving through air, where mixing at the boundary is continually occurring, is probably impossible to predict without experiments. In any case, it seems unlikely that the form drag on a cloud could exceed that on a comparable solid cylinder when the higher drag coefficient is used. For draughts of diameter 1 km, moving through the air at 2 m/s, Eq. (8) gives a form-drag force ranging between $0{\cdot}7 \times 10^6$ to $3{\cdot}6 \times 10^6$ dynes per cm of vertical extent for a C_D between 0·3 to 1·5. The force due to normally developed entrainment for such a draught rising at a few m/sec lies roughly in this range also. Since some of the environment air is moving ' through ' the draught rather than around it, the solid cylinder drag may be expected to set an upper limit for the form-drag term. A rough calculation made for the same draught as that of the previous example shows that entrainment and detrainment may move a mass of air through the draught per second of comparable magnitude to that displayed by it. Therefore the form-drag term may range from totally negligible up to equal magnitude with the force due to entrainment.

In view of the above discussion an expression for f proportional to $(u_E - u_c)^2$ should be substituted into Eq. (4). This has been done and the solution to the resulting non-linear equation has been obtained. From that solution it is readily shown that for reasonable values of the friction forces and other parameters, only very small differences

in u_c result in the first km or more above cloud base between an f of the quadrat form and an f linearly dependent on $(u_E - u_c)$, namely,

$$f = Q'(u_E - u_c) \quad . \quad . \quad . \quad . \quad . \quad (9$$

where Q' is adjusted so that Eq. (9) gives the same friction force as the non-linea expression at cloud mid-level. It is probably worth sacrificing slight refinements i the form-drag term for this simplification, since so very little is known about forn drag on convective draughts at present that even an estimation of its order of magnitud would represent a significant step.

(c) *Solution of the equation*

Upon substitution of Eqs. (6), (7), and (9) into Eq. (4) it becomes

$$\frac{du_c}{dz} = (u_E - u_c)\,P + Q\,(u_E - u_c) \quad . \quad . \quad . \quad (10$$

where $Q = Q'/\overline{M}$. A tentative expression to estimate the order of magnitude of may be derived from combination of its definition with Eqs. (8) and (9). It stat that

$$Q = \frac{2C_D\,(u_E - u_c)}{\pi\, d\, w} \quad . \quad . \quad . \quad . \quad (11$$

If now du_E/dz is subtracted from both sizes of Eq. (10) and $(u_E - u_c)$ is calle v, Eq. (10) simplifies to

$$-\frac{dv}{dz} = (P + Q)\,v - b \quad . \quad . \quad . \quad . \quad (12$$

when relation (5) is also introduced.

The solution of Eq. (12) is readily found to be

$$c\,(Rv - b) = e^{-Rz} \quad . \quad . \quad . \quad . \quad (13$$

where c is the constant of integration and $R = P + Q$. It is easily shown that equ form drag and entrainment frictional forces give equal P and Q. The most gener boundary condition for evaluation of c states that at some level l above draught ba $(u_E - u_c) = (u_E - u_c)_l = v_l$. Therefore, from this level upward

$$v = (u_E - u_c) = \frac{b}{R}\left[1 - e^{-R(z-l)}\left(1 - \frac{R}{b}\,v_l\right)\right] \quad . \quad . \quad (14$$

By successive applications of Eq. (14) the relative cloud-air horizontal velocity can b determined for the entire vertical extent of a draught. From its base up to the fir level where R or b changes, the equation has a particularly simple form, since if $l = v_l = 0$.

The cloud slope can finally be written

$$\tan\theta = \frac{w}{(u_c - u_{E_0})} = \frac{w}{b\left[z + \frac{1}{R}\,(e^{-Rz} - 1)\right]} \quad . \quad . \quad (15$$

when average values of b and R are taken. It is readily obtained and only slight more complicated when R and b change at one or more upper levels.

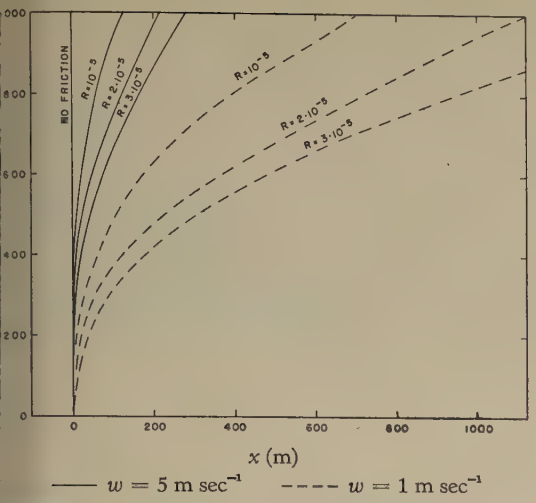

Figure 2. Horizontal draught displacements as a function of height for varying vertical speeds and friction forces when the wind shear is 5 m sec^{-1} km^{-1}. Calculation made from Eq. (15).

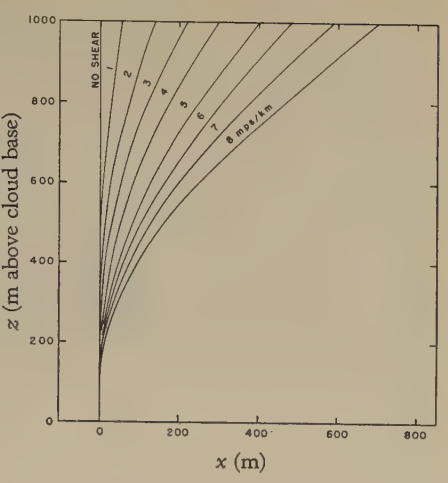

Figure 3. Horizontal draught displacements as a function of height for varying wind shears when the ascent rate is 3 m sec^{-1} and the friction coefficient $R = (P + Q)$ is 2×10^{-5} cm^{-1}. Since the slant varies inversely as w, these curves would each correspond to one-half the shear when w is halved, etc. Calculation made from Eq. (15).

It is seen immediately from Eq. (15) that the model predicts clouds which slant in the postive x or forward direction when the x-component of the wind increases with elevation, and backwards when the wind decreases upward, the slant increasing in direct proportion to the shear. Moreover as long as the cloud remains in a steady state and its updraughts are neither strengthening nor weakening with time, *the slope remains constant*. The fact that real clouds are observed to become more slanting with time is due to the gradual weakening of their updraughts and is discussed presently. Eq. (15) shows that the slant increases linearly as w decreases, if one assumes roughly that the decay can be represented by a series of quasi-steady states of smaller and smaller w.

Figs. 2 and 3 show sample calculations made from Eq. (15). Fig. 2 takes a constant, moderately large external shear of 5 m sec^{-1} km^{-1}. The slopes of the clouds are then predicted for varying total average frictional forces and mean vertical speeds. For equal shear and vertical speed, the increase of cloud slant with increased friction is found to be sub-linear, the more so the larger the frictional forces. However, since increased horizontal friction of the type due to entrainment, at least, implies increased resistance to vertical cloud growth and hence reduced upward speeds, the slope in real clouds may depend far more sensitively on R (via P) than would appear from first glance at Fig. 2. Also, real clouds will slant more near their bases than the theoretical clouds because there the vertical velocities will be smaller than in their central portions.

Fig. 3 shows the effects of normally observed wind shears on cumulus having vertical speeds of 3 m/sec – of the order found in swelling cumulus in mid-latitudes and probably in most trade cumulus (see Stommel 1951). Shears in the trade-wind cloud layer usually range from 1 m sec^{-1} km^{-1} to 3 m sec^{-1} km^{-1}, while in the corresponding layer in mid-latitudes they average 5 m sec^{-1} km^{-1} to 7 m sec^{-1} km^{-1}. Trade cumuli, then, for the same vertical speeds may be expected to slant considerably less than their mid-latitude counterparts.

(d) *Effect of slant upon cloud dynamics and life cycles*

It has been shown by the *Thunderstorm Project* (see Byers and Braham 1949 Chap. V) that cumulonimbus clouds grow by putting up successive towers at or nea their tops. Observations presented in the next section show that smaller cumulu also behave in this manner. If the wind shear is weak, the cloud turrets will remain nearly vertical even as their upward velocities decay and they dissipate. The new turrets can then grow into and entrain air moistened by their predecessors. When on the other hand, the external shear is strong, the dissipating towers become very strongly slanting and the new vigorous towers, which grow much more nearly vertically are penetrating dry air. To obtain a quantitative idea of this effect, reference is made to Fig. 3. If the external shear is, for example, 1 m sec^{-1} km^{-1}, the slant of the towe increases from that of the line marked '1' to the line marked '2' as the vertica velocity decreases to $1\frac{1}{2}$ m sec^{-1}. The slant has only increased to that of the lin marked '4' when the cloud is in such an advanced stage of dissipation that its ascen rates are only 75 cm/sec. If, in contrast, the shear is 6 m sec^{-1} km^{-1}, the tower slant along the line marked '6,' reaching '8' when the updraught speed has only declined to $2\frac{1}{4}$ m sec^{-1}. This example illustrates one of the several reasons why trade cumul grow so readily, day and night, with smaller turbulence and perhaps less require buoyancy for given vertical speeds compared to their mid-latitude counterparts.

The model just presented may also be used to calculate the frictional forces on clouds from their observed slopes. If a mean vertical velocity at several levels wer known, as well as the external wind shear, the friction forces could be calculated, leve by level, from photographs showing cloud slope. If, in addition, temperature and wet-bulb readings were available inside and outside the cloud at these same levels one would have all the information necessary to calculate gross entrainment by Stommel' method, the net entrainment to fulfil continuity, the total horizontal friction forc from the slope, and the horizontal form-drag force from the difference between th entrainment force and the total horizontal friction force. Unfortunately, such simultaneous data are so far lacking. Various less complete sets of observations ar however, available, and are sufficient for qualitative verification of the foregoing predictions and to suggest further lines of investigation.

3. Observations

(*a*) *First case – an example of trade cumulus.* Fig. 4 (Plate III) shows a photograph of oceanic trade cumuli in the region of San Juan, Puerto Rico, on the afternoon o 25 Apr. 1946. It was taken by the Wyman-Woodcock (Woods Hole Oceanographi Institution*) Caribbean expedition from an altitude of about 18,000 ft. The wind (from ESE.) blows from left to right in the plane of the picture and decreases upwards The clouds clearly slope backwards, in qualitative agreement with Eq. (15). Fortunately temperature and wet-bulb soundings were made inside and outside one of the cloud in the area. The gross entrainment and vertical velocities in this cloud have been calculated from the soundings by Stommel (1951) and are reproduced in Table I along with the present calculations.

In computing these vertical velocities, Stommel assumed, among other things that the entrainment calculated from the soundings was the only resistance forc

* Sponsored by the U.S. Navy's Bureau of Ships and Office of Naval Research. Reported on by Bunker, Haurwitz, Malku and Stommel (1949).

Figure 4. Photograph of trade cumulus north-east of Puerto Rico (20°00′N, 64°25′W) on the afternoon of 25 Apr. 1946 taken from 18,000 ft altitude. The camera was pointed toward a direction just west of south, at right angles to the ESE. wind, which decreases upwards. The wind therefore blows from left to right across the photograph. The soundings discussed in the text were made within and near one of the clouds in the area. Cloud base is at about 600 m, tops about 1,200 m to 1,400 m.

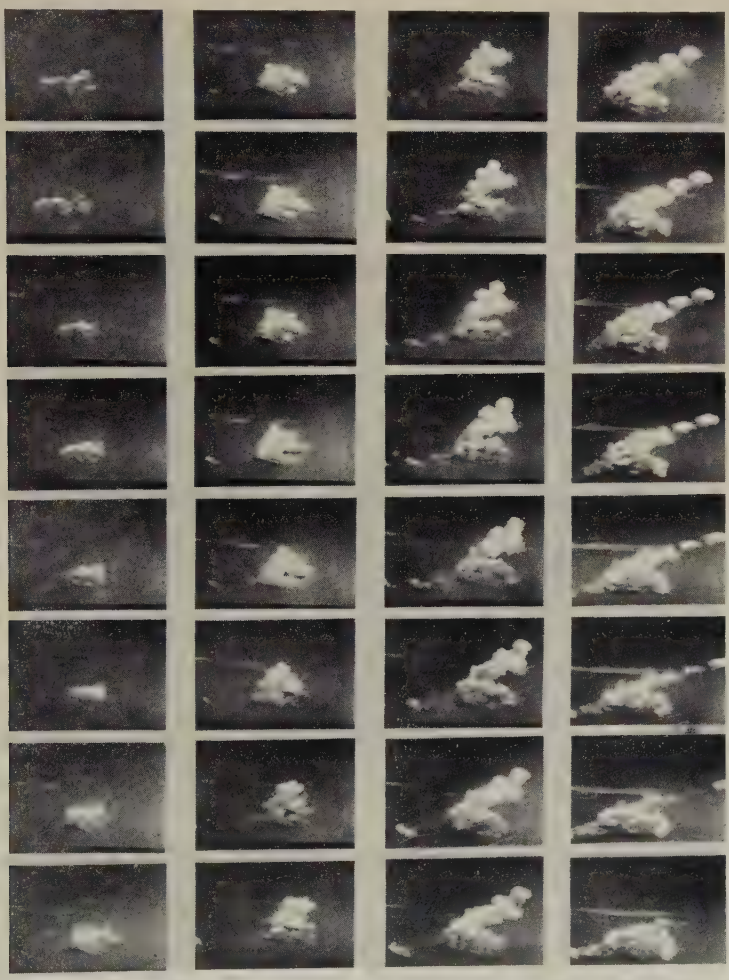

Figure 6. Sequence showing the life history of a cloud turret over Nantucket Island (Massachusetts, U.S.A.) on 14 Aug. 1950. Frames are reproduced every 30 secs, beginning in the upper left. The horizon is visible as a dark line in the foreground. The camera was pointed nearly due east, at right angles to the surface wind which was from the south (right), decreasing rapidly upward. Cloud base was at 450 m and the highest level reached by the top was just over 2 km.

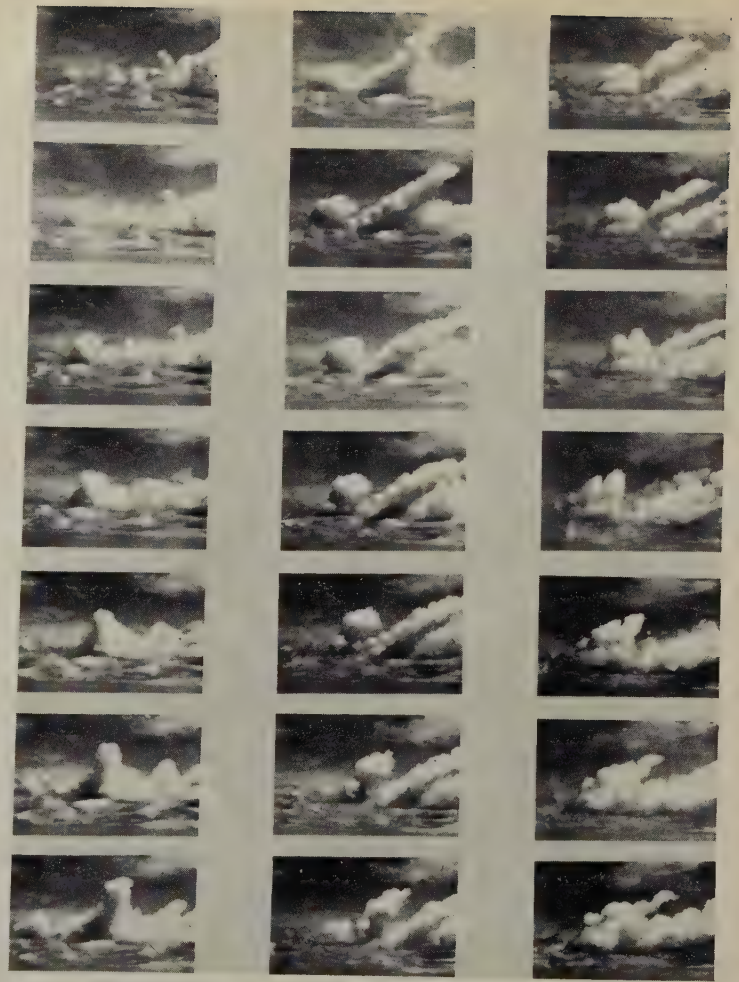

Figure 8. Sequence showing the life histories of several clouds over Nantucket Island on 8 Aug. 1950. Frames are reproduced every 30 sec beginning in the upper left. The horizon is not visible, but five radio towers were visible on the original prints, confirming reported locations and directions. The camera was

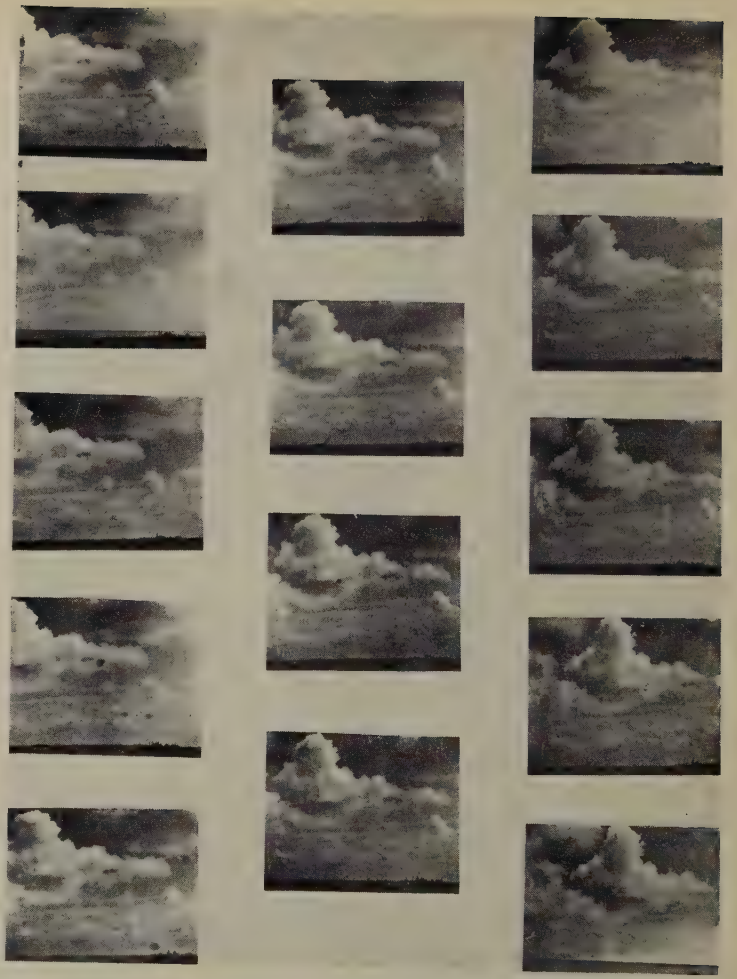

Figure 10. Sequence showing a cloud tower over Nantucket Island on 5 Sept. 1950. Frames are reproduced at 6-sec intervals. The camera is pointed east, at right angles to the surface wind which was from the north (left) and constant

through the cloud layer. Note the vertical growth of the clouds

operating against the growth of the clouds. It is interesting to check his theory by calculating the horizontal cloud speed from the theory presented here under the same assumption (namely that $Q = 0$, and so in Eqs. (14) and (15), $R = P$), then put this together with Stommel's calculated vertical motions to predict the cloud slope, which can finally be compared with Fig. 4. The other assumptions made by Stommel in arriving at the figures shown in Table I were identical with those used here in deriving Eq. (15), except that he assumed that the outside air mixed with the cloud air remained within the updraught, or that dM_0 was zero. His data permit the calculation of $P = \frac{1}{M}\frac{dM_i}{dz}$ for several height intervals without any assumption concerning how much of the entrained air was retained.

TABLE I.

z m above sea level	mass flux (in arbitrary units) if all air retained	vertical speed (m/sec)	$\frac{1}{M}\frac{dM_i}{dz} = P$ (calc.) $\times 10^5$	b (obs) $\times 10^3$	$(u_c - u_{E_e})$ m sec^{-1}
610	1·0	0			0
757	1·2	2·4	1·3	− 4·3	− ·06
915	1·7	3·6	2·8	− 4·3	− ·39
1070	2·1	2·9	1·6	− 4·3	− ·65
1220	3·2	0	3·5	0	− 1·18
			$P_{ave} = 2·3$		

Fig. 5 shows the calculated slope of the cloud. The slant is in the same direction (upwind) but somewhat less than that of most of the clouds photographed. The tilt of the actual clouds cannot have been exaggerated by more than 5 per cent by the altitude from which the picture was taken, since even those in the foreground were about 70,000 ft from the camera. In carrying through the individual steps of the calculation shown in the table, one finds that the only reasonable way in which the slope of the theoretical cloud could be significantly increased is by reduction of the vertical ascent speeds in its lower and central portions. It is not known in which, if any, of the depicted clouds the sounding used by Stommel was made and it may have been that most of those shown were in a later stage of their life cycles, or it may be that Stommel's method overestimates vertical velocities since it assumes that entrainment is the only resistance to the vertical growth of clouds. This question cannot be answered until photographs simultaneous with soundings are made of the same cloud. It should be noted that the wind shear on this day was considerably greater than normal for the trade-wind region, being 4·3 m sec^{-1} km^{-1}, a value more common in middle latitudes. The average value of P throughout the cloud layer came out to be $2·3 \times 10^{-5}$ cm^{-1}, noticeably greater than the average found by Stommel (1947) for trade cumulus (five cases, mean $P = 1·5 \times 10^{-5}$ cm^{-1}). In addition to stronger than normal decrease of easterlies in the cloud layer, the trade inversion was considerably lower than normal (base $\simeq$ 1200 m) and the cloud top had penetrated the drier, more stable air, as is seen from Stommel (1951) which shows negative buoyancy in the upper 330 m of the cloud. Except for these upper 330 m, the lapse rate through the cloud layer was 0·68°C per 100 m, and the mean mixing ratio 13·0 g/kg. Near the upper portions of the cloud, the lapse rate was 0·33°C per 100 m and the mixing ratio just under 5.0 g/kg. In this case, the cloud growth was clearly stopped by penetration of a stable dry layer, which served as a lid upon convection.

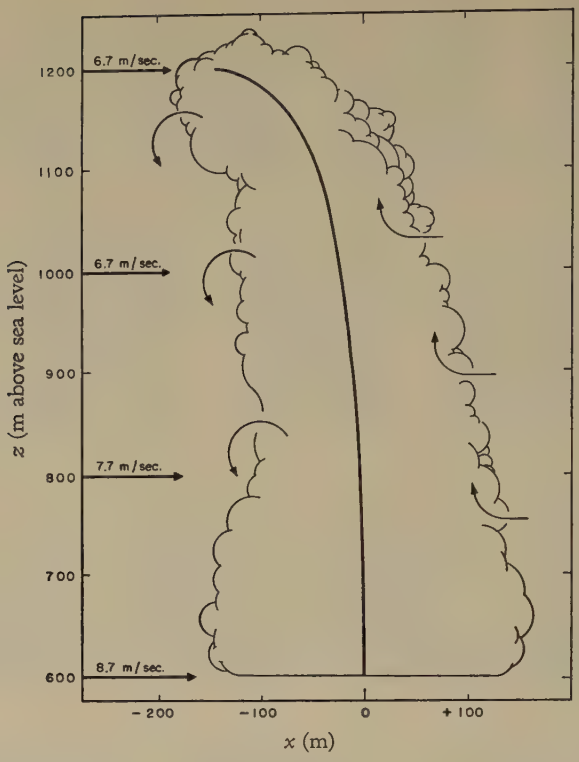

Figure 5. The theoretical slant of the trade cumulus of 25 Apr. 1946 studied by Stommel (1951) is give by the dark solid line. The dark arrows to the left are the observed external winds at San Juan at noo local time. The light curved arrows represent schematically the hypothesized air motions into and o of the cloud. The ' detrained ' air is shown descending outside the cloud in conformity with time-lap motion pictures of cumulus.

(*b*) *Second case – an example of middle-latitude cumulus with strong negative shea* Fig. 6 (Plate III) shows a series of time-lapse motion pictures made of a cloud forme over Nantucket Island, Massachusetts on 14 Aug. 1950. The photographs are reproduce at 30-second intervals. The camera was pointed in a direction normal to the surfac wind, which was from nearly due south and decreased rapidly upwards. The averag shear through the cloud layer was 5 m sec^{-1} km^{-1}. Aeroplane records (see Malku and Bunker 1952) gave the height of the cloud bases (450 m) and their location wit respect to the camera. This knowledge permitted the rate of rise of the cloud tow to be calculated from the film and hence an estimate of the ascent rates within th cloud. The tower reached its greatest height in the 23rd frame or 11½ minutes aft it began to grow, giving a mean rate of rise of about 2 m sec^{-1}. Between the 19th an 21st frames the tower ascended at an average rate of slightly more than 3·0 m sec^{-1}.

The observed slope at one-minute intervals beginning with the 20th frame ha been plotted in Fig. 7. Assuming a mean vertical draught speed of 3 m sec^{-1} prevaile at this time, the slope could be exactly duplicated from the theoretical model if a tot frictional coefficient of $R = 2{\cdot}5 \times 10^{-5}$ c/m^{-1} were taken as the mean value for th entire cloud and if it were assumed that the updraught began at 150 m below th cloud bottoms and increased linearly up to 3 m sec^{-1} at cloud base. From the observe vertical speed in the 20th frame (w_0 in Fig. 7), vertical speeds for the 22nd, 24th, 26t

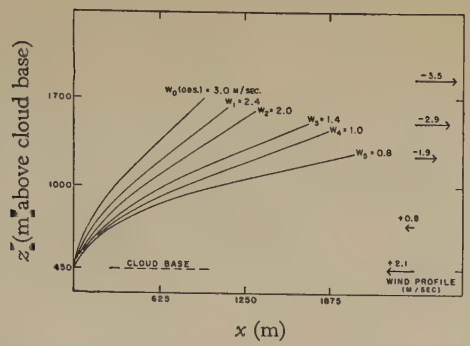

Figure 7. Observed slant of cloud (14 Aug. 1950) depicted in Fig. 6 at one-minute intervals beginning with frame 20. The vertical speed w was observed from the film; those remaining (w_1, w_2, etc.) were calculated from the slopes. The pilot-balloon observation made within a few kilometres of the clouds is reproduced on the right.

28th, and 30th frames (w_1, w_2, w_3, w_4, and w_5, respectively) could be calculated from the observed slopes shown in the figure, using the same value of R. The decay of the draught, and all the other computed values seem to present a reasonable and consistent picture. Concerning the relative contribution to R by form drag and entrainment, it is important to note that the upper limit of Q in this case is probably about $0{\cdot}4 \times 10^{-5}$ cm^{-1}. This coefficient was computed from Eq. (11) using $C_D = 1{\cdot}5$, together with the observed cloud diameter, calculated relative speeds and vertical motions. This sets a lower limit for P, the fractional rate of entrainment, as $2{\cdot}1 \times 10^{-5}$ cm^{-1}, a value of the same order of magnitude but about 40 per cent larger than the mean rate inferred by Stommel. In the present example, the horizontal motion of the cloud was so small, as was the wind speed, that an accurate calculation of the relative cloud-air speed from the film was not feasible.

It is interesting to note that this cloud was able to put up only one tall tower, reaching barely over 2 km, although abortive attempts to raise at least two others from its left side are obvious from the photographs. The cutting off of the one tall tower in the region of strongest shear (1,000 m to 1,500 m) is also noteworthy. Throughout the entire cloud layer and far above its top, the lapse rate maintained a nearly constant value of 0·69°C/100 m, so that it is not possible to ascribe either the limitation of the cloud's growth or the cutting off of the tower to any stabilization of the air. It is concluded that this cloud was curtailed and eventually killed by entrainment of drier air, especially since the parcel method shows vast amounts of energy available for its growth up to at least 4 km (the radiosonde and other details of this case are discussed by Malkus and Bunker 1952). The observed moisture distribution supports this conclusion. In the subcloud layer the mixing ratio averaged nearly 10 g/kg; from cloud base to 1 km, it averaged 7·1 g/kg, from 1 km to cloud top at 2 km its mean value was 5·4 g/kg and from 2 km to 4 km, 3·8 g/kg. It is inferred that strong wind shear gave rise to both a high entrainment rate and a strongly slanting cloud, which factors coupled with a very dry environment served to inhibit the cloud's growth.

(*c*) *Third case – an example of middle-latitude cumulus with weak positive shear.* Fig. 8 (Plate IV) shows another series of time-lapse motion pictures made over Nantucket Island on 8 Aug. 1950, and again reproduced at 30-second intervals. In this case, the wind remained from due north at all levels (from left to right across the plane of the photographs) and increased upward. The shear was + 3·0 m sec^{-1} km^{-1} from 600 m to

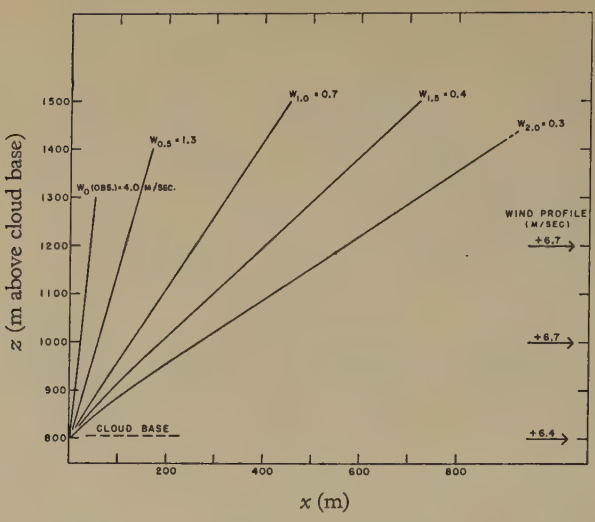

Figure 9. Observed slant of cloud (8 Aug. 1950) depicted growing in frames 1-7 of Fig. 8 at half f minute intervals. The vertical speed at the 6th frame, w_0, was observed from the film, those remaining were calculated from the slopes. The pilot-balloon observation made within a few kilometres of the clouds is reproduced on the right.

900 m, and nearly zero from there up to 1,200 m. Cloud base was observed from the plane to be at 800 m, and the cloud location was also specified in this manner. The calculations were made for the first cloud tower in the series, which commenced to grow actively in the 2nd frame, reached its maximum elevation in the 10th (after 4 min) and 3 min later had all but disappeared. The average rate of rise of this tower was 4 m sec^{-1}, and this differed little from one frame to the next. The maximum rate of rise occurred between the 5th and 6th frames. The observed slopes for the 6th to the 10th frames are plotted in Fig. 9.

The curve for the 6th frame was exactly duplicated from the theoretical model under two assumptions : first, that the average total friction coefficient R was equal to $2{\cdot}0 \times 10^{-5}$ cm^{-1}; and second, that the updraught began at 300 m below cloud base, increasing linearly up to 4 m sec^{-1} at 100 m above cloud base. From these same assumptions, the observed slopes at half-minute intervals for the 7th to the 10th frames were found to correspond to the rate of updraught decay shown in Fig. 9. Note the very much more rapid decay than in the preceding case. Owing to the greater windspeed on this day, it was possible to calculate fairly accurately the relative horizontal cloud-air speed from the films. These calculations showed that the central portions (at about 1 km elevation) of the clouds were moving downwind at approximately $1{\cdot}2$ m sec^{-1} slower than the external wind. The relative velocity obtained from theoretical considerations was $1{\cdot}3$ m sec^{-1} at 1,100 m, in excellent agreement.

The lapse rate from 800 m to 1,500 m (cloud layer) on 8 August was given by the Nantucket radiosonde as about 0·5°C/100 m, steepening slightly above this. No very stable layers or inversions were observed. The mixing ratio averaged 7·1 g/kg in the cloud layer and had fallen to 5·5 g/kg at 2 km. Computations by means of the parcel method showed only a rather small positive area within the cloud layer, with the moist adiabat recrossing the sounding at about the height of the observed cloud tops. Therefore the lack of continued cloud growth is readily understandable.

q. (11) shows that the maximum value of Q would be about $0{\cdot}8 \times 10^{-5}$ cm^{-1}, using $\rho = 1{\cdot}5$. This means that the minimum value of the fractional entrainment rate, is $1{\cdot}2 \times 10^{-5}$ cm^{-1}.

(*d*) *Fourth case – an example of middle-latitude cumulus with negligible shear.* Fig. 10 (plate IV) shows a series of Nantucket clouds photographed around noon on 5 Sept. 1950. These are reproduced at 6-second intervals. The cloud base averaged 600 m and at this time of day most of the tops terminated at about 900 m. From 300 m to 1 km the wind was constant in both speed and direction, 8·9 m sec^{-1} from 360°, or from left to right across the photographs. From 1 km upward it decreased, becoming 7 m sec^{-1} at 1,965 m.

Since the clouds in the photographs were developing vertically, as predicted, the frictional forces could not be computed. The mean ascent rate of the cloud tower shown was slightly greater than 1 m sec^{-1} during this short period. The slow rise of the turret was probably due to the fact that it had already penetrated the small inversion at 900 m, to be discussed presently. The horizontal speed of this cloud came out, within computational error, to be exactly that of the wind.

The aeroplane sounding over the island (see Malkus and Bunker 1952) made just before noon showed a lapse rate of 0·59°C/100 m from cloud base up to about 900 m, at which level a small inversion occurred. The average mixing ratio for 600 m to 900 m was 7·8 g/kg. The parcel method showed positive energy from cloud base up to the small inversion and, if that rather small negative area were surpassed, more positive energy up to a very strong inversion at 2·1 km. The temperatures inside these clouds were higher than outside near their bases and lower than outside near the tops where they had penetrated the small inversion.

Succeeding series of time-lapse pictures (not reproduced owing to poor photography) continuing into the afternoon showed an interesting sequence of cloud development. At first the cloud tops were cut off in the small inversion and dissipated directly above their lower portions. Later, successive towers of the same cloud penetrated higher and higher, until it was clear that they had destroyed the small inversion. Around 1425 EST the aeroplane observer noted towers reaching 1,500 m to 1,600 m, and slanting strongly backward as they penetrated the region of negative wind shear. Observations made within these clouds showed their tops to be warmer than the environment air. No towers were noted which reached the base of the large inversion at 2·1 km. This case was considered to be a particularly valuable one, as it showed clouds first being stopped by, then destroying, a small stable layer, then being inhibited by friction and entrainment of drier air aloft.

4. Conclusion

It has been shown by means of the very simple and unequivocal measurement of cloud slope that entrainment or mixing occurs in several examples of cumulus clouds of the same magnitude inferred by Stommel (1947) from aeroplane-psychrograph soundings. It can hardly be doubted any longer that interaction of a significant degree takes place between cumulus clouds and their surroundings and plays an important, frequently vital, role in the dynamics of the clouds, at least in their small and swelling cumulus stages. The balance achieved between vertical motions and mixing controls the release of latent heat by condensation (which in turn exerts a reverse control on vertical draughts and mixing) and hence even the microphysics of clouds, drop-growth rates and size distributions, may be expected to be markedly influenced by such macroscopic external variables as wind shear.

This paper does not purport to answer the very serious questions concerning t mechanisms of ' entrainment,' whether it consists mainly of an ordered inflow, or o turbulent mixing process resembling that in a heated jet, nor does it tell much concern the functional dependence of entrainment rates upon the other variables. Furth observations of the type suggested here, including those of cloud slopes, howev may be expected to provide this information. In concluding, it must be emphasiz that a cloud is a process, not an entity like a pilot balloon. What we call a sin cloud may completely exchange its air several times during its life cycle. Futu observational work must, therefore, be planned to obtain a wide variety of measureme within the cloud and to relate them to the structure of its environment. To this e high refinement in individual observations is at the present stage probably wo sacrificing.

Acknowledgments

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She would also like to acknowledge the contribution made by Dr. Patricia Langwell, in collaboration with whom her work on cloud slopes was initiated.

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The production of showers by the growth of ice particles

By F. H. LUDLAM
Imperial College, London

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SUMMARY

A model of cumulus-cloud structure is described, and used, together with the available evidence on the temperatures of ice-crystal formation and a calculation of the speed of growth of crystals into precipitation elements, to estimate the summit-level temperatures at which the formation of showers becomes likely. It appears that in warm weather showers may be expected with summit temperatures of about −10°C, but in cold weather not until they fall to about − 20°C. Observational evidence is shown to support this view although emphasizing large casual variations, some of whose causes are suggested.

1. INTRODUCTION – THE STRUCTURE OF CUMULUS

In a previous paper (Ludlam 1951, subsequently referred to as I) the conditions under which shower rain may form by the coalescence of cloud droplets were examined. In the present paper the formation of showers by the growth of ice particles appearing in the supercooled parts of cumulus is considered. It is necessary first to consider the large-scale features of cumulus, and how they determine conditions which must be fulfilled before shower production can begin.

The careful observation of cumulus and the experience of glider pilots in using the updraughts both in and beneath the clouds suggests that they contain rising ' bubbles ' of buoyant air which have a diameter of about a kilometre. These bubbles seem to have well-defined upper surfaces, but in constructing a model of the cloud structure we may suppose them to have extensive wakes in which there is mixing with the outside air. At any moment a large cloud may contain a number of bubbles and for the rest be composed of their wakes or the residue of earlier bubbles, as suggested schematically in Fig. 1. Once a bubble reaches the outer parts – the summit or the flanks – of the cloud the mixing and wastage into a wake is accelerated ; the rapid evaporation at the cloud borders chills the air there and destroys its buoyancy or even produces sinking, whereas in the interior of the cloud a bubble is to some extent

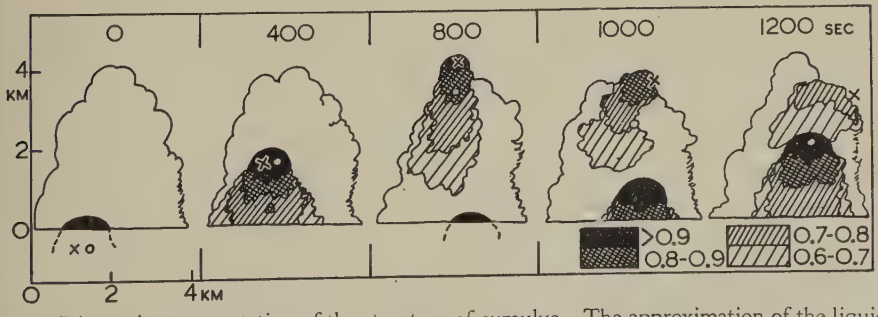

Figure 1. Schematic representation of the structure of cumulus. The approximation of the liquid-water content to that in air rising from the base *unmixed* is indicated for two successive rising ' bubbles ' of cloud air. The successive positions of two cloud particles, one of which grows into a precipitation element, are also shown.

protected and is able to rise much farther than if it ascended through clear air. cumulus therefore has a characteristic pyramidal shape, and viewed from a dista appears to have an unfolding motion, fresh cloud masses continually emerging fr the interior to form the summit and then sinking aside and evaporating. The size the cloud depends not only upon the properties of the environment, for exam temperature and dryness, but upon the rate of supply of warm air bubbles into cloud base, which is determined by the rate of heating of the surface air and oth factors whose study has hardly been begun.

In Fig. 1 is indicated hypothetically the approximation of the liquid-water cont to the value corresponding to ascent from the cloud base without mixing. The stren of the updraught is similarly distributed, and it is clear that an aircraft making a hig speed traverse of the cloud experiences 'turbulence' in passing through seve regions of varying updraught, whereas a glider pilot ascending in a region of maxim updraught regards the cloud air as 'smooth,' for during the time the glider takes sink at about 1 m/sec completely through a bubble it may be lifted into the clo summits. In considering the *formation* of precipitation it can be assumed that the fi precipitation elements are grown in the bubble cores ; whether or not these cont some unmixed air throughout the rise to the cloud summits is at present a mat of conjecture. However, it appears from an analysis of aircraft soundings (not published), that the tops of *large* cumulus (more than 2 km deep) are usually fou at about the level air would be expected to reach if it followed the saturated adiaba with respect to liquid water, if allowance is made for the weight of suspended wat Thus in considering the formation of showers in large cumulus it seems justifiable assume that the precipitation elements begin their growth in limited volumes of which rise without mixing from the cloud base to near its tops (measurements cumulus liquid-water content so far published suggest considerable mixing but a unlikely to be representative of conditions in the bubble cores inside large cloud however it should be noted that we assume the most favourable conditions for show formation, and that the effect of any mixing must be to increase our subsequent estima of the size of a cloud which is capable of producing a shower).

In Fig. 1 are marked the positions within a bubble of two particles which insi the cloud begin to grow into precipitation elements : these may be regarded as lar condensation nuclei which grow within the cloud by coalescence, or as ice nuclei whi grow into hailstones in the supercooled part of the cloud. One of these partic grows faster, being either larger initially or active as an ice nucleus at a higher temperatu and is shown as settling out of the air which carried it into the cloud : it remains in t interior of the cloud, enters a newly-rising bubble and completes its growth into raindrop or hailstone. Meanwhile the other fails to acquire a substantial fall-spe before being carried to the outside of the cloud, where it is quickly evaporated togeth with the neighbouring cloud droplets. In order to become a precipitation element particle must reach some rather critical size during the time taken for cloud air rise from the base to the summits, such that it shall have settled some hundred metr or more relative to the air, gained a fall-speed about equal to that of the updraugh near the cloud top ($\simeq$ 1 m/sec), and have the capacity for falling with insignifica shrinking through several hundreds of metres of unsaturated air – and hence th opportunity to survive even if caught in the general evaporation of a cloud tower, ar so to resume its growth by settling into a newly-rising mass of air within the cloud bul It so happens that these conditions are simultaneously fulfilled when the particle h

fairly well defined size of some hundreds of microns ; it was previously shown that or a droplet the corresponding radius is about 150 μ, and that the condition that roplets of this size shall be produced before cloud air reaches the summits determines, n association with the updraught-speed, a rather critical summit-level which must be ached before shower formation by coalescence begins. If there is a considerable hange of wind with height the cloud tops may project beyond the base, so that any rge particles settling out of them fall into clear air rather than into succeeding bubbles nd the development of rain is still prevented. Moreover, such a wind shear makes it less kely that the bubbles rising from particularly favoured source-regions ascend through ne wake of their predecessors, and thus exposes them to more rapid desiccation. Iowever, in the present paper we overlook such inhibiting effects in estimating the ummit-levels at which the onset of showers by the growth of ice particles becomes kely ; first we consider the level at which a concentration of ice crystals sufficient or shower development occurs, and then how rapidly these crystals grow into ice articles of the critical size, i.e., into precipitation elements, assuming their growth o take place in air which has not been considerably mixed with outside air during s rise from the cloud base.

2. Atmospheric ice nuclei

If a steady state is assumed in the supply of ice nuclei to a cloud by an updraught f some m/sec, and in the fall of raindrops grown from them to a radius of 1 mm, hen a moderate rate of rainfall (several mm/hr) is produced if the nuclei have a oncentration of 100/m^3. This is a reasonable estimate of the concentration required or shower formation ; it may be reduced by an order of magnitude if in a big cloud ach crystal can eventually become a large raindrop, and perhaps by several orders of nagnitude if the cloud can support a chain reaction of raindrop multiplication.

There have been few determinations of the temperatures at which such small oncentrations of atmospheric ice nuclei become active, and only the observations of indeisen and Schulz (1944) are directly applicable. According to their experiments in clouds made by expansion of surface air at Prague) the temperatures at which articular concentrations are active vary somewhat from day to day and markedly ith the rate of expansion during the initial condensation. At a rate corresponding o an upward speed of 5 m/sec in the atmosphere a concentration of 100/m^3 was usually bserved at about — 14°C ; this temperature fell as the speed was increased, to become bout — 25°C at 40 m/sec, and then rose again towards its former value as the speed pproached 100 m/sec (Schulz 1947). Probably on account of this reversed trend neasurements by Palmer (1949) at speeds of 140 m/sec to 280 m/sec roughly agree ith those of Findeisen and Schulz at 15 m/sec. This variation in the activity of ice uclei with rate of expansion has not yet been explained, but is important, for whereas he updraughts at cumulus bases rarely exceed 5 m/sec (see, e.g., Pielsticker 1940), he equivalent speeds necessarily employed in small expansion chambers and cold oxes amount to several hundred m/sec. Consequently there is doubt about the ignificance of observations made, for example, by Schaefer (1950) and Workman nd Reynolds (1949). The measurements of aufm Kampe and Weickmann (1951) nay be more representative, since they used a very large cold chamber in which much f the air may have been chilled by mixing at a slower rate ; they observed a concentration f 100/m^3 of active nuclei at about — 10°C.

Although it may be inferred that this concentration is usually active in cumul at temperatures between about — 10° and — 14°C, there is almost certainly a gr variation from place to place, and the threshold temperature might be expected to considerably lower in oceanic air than in the relatively polluted air over continen although the sea-spray which is commonly formed in unstable maritime air may be source of efficient ice nuclei. Clearly further observations of the occurrence a and variation of natural ice nuclei are desirable, and because of the difficulty of operati sufficiently sensitive counters it may prove easier to observe from aircraft the concentr tions of crystals which actually arise in supercooled cumulus summits. Coons, Jon and Gunn (1949), for example, state that crystals can usually be detected visually the tops of Gulf States cumulus if the summit-level temperature is below — 6° although apparently they do not always lead to shower formation. The occurrence persistent *stratiform* clouds at temperatures even below — 20°C is not evidence of t absence of ice nuclei, for these clouds are stable to infection with crystals in the sm concentrations sufficient for shower production, containing feeble updraughts whi soon permit the growing crystals to fall out of the cloud and evaporate.

For the present we assume that inside cumulus sufficient crystals usually appe at about — 10°C, and estimate their growth at temperatures between this and abo — 20°C.

3. The growth of ice crystals into precipitation elements

It is now accepted that the ice nuclei active in the atmosphere at these temperatur are freezing nuclei ; such a nucleus causes a cloud droplet to change almost instant into a sphere of ice with some enclosed liquid which freezes more slowly. Vapo then condenses on the surface, which contains a number of crystal faces ; the furth growth is likely to be irregular, but it seems reasonable to regard the particle as remainin spherical. In the high supersaturation over ice which prevails the particles gr rapidly and soon gain a size and fall-speed such that their growth continu predominantly by the capture and freezing of droplets.

To estimate the growth it is necessary to consider the density of the added materia At first, while the condensation is more important than collision with droplets, th density is likely to remain at about 0·9, but as the coagulation begins to predomina the quickly-freezing droplets are likely to include air enclosures: we shall at fir assume that when the mass of ice added by the coagulation exceeds that deposit from the vapour the density of the added material is 0·6. When the radius of th particle exceeds about 200 μ (and its fall-speed is rather more than 1 m/sec) condition are favourable for dendritic growth of the surface irregularities (Mason, unpublished see also Mason and Ludlam 1951), and the density of the rime deposited thereaft is likely to be very low. The rime is composed partly of ice condensed from the vapou into dendritic sprouts, and partly of frozen droplets. In the early stages of the growt of a hailstone these droplets arrive at its surface with relatively small speeds an probably freeze very rapidly on the tips of the dendrites, so there is no flow of liqui into the interstices and the rime contains a large amount of enclosed air. The proble of its density is analogous to that of the density of the rime which forms on a rotatin cylinder in a supercooled cloud, which has been discussed by Langmuir (1944 He derived empirical formulae relating the density to the air temperature and the siz and impact speed of the cloud droplets, which suggest a density less than 0·1 for sma coagulation elements at temperatures below — 10°C. A similarly low value is indicate

y a theory of hailstone growth which will be described in a future paper. Densities f 0·05 to 0·1 commonly occur when rime is deposited in conditions favouring dendritic rowth of the ice surface and when the rate of arrival of the droplets is rather small, s on large cylinders having a small efficiency of catch, or in clouds of small liquid-vater content (see, e.g., Melcher 1952). The only published measurements of the lensity of small hailstones appear to be those of Nakaya and Terada (1935), who ound an almost constant value of 0·13 indicated by the fall-speeds of graupel of radius to 3 mm. The present writer similarly estimated a value of about 0·1 for small raupel of radius about 400 μ which fell in a winter shower. In view of this evidence ve assume that when the radius of the growing ice particle exceeds 200 μ the density f the added material is suddenly reduced to 0·1, making comparative estimations vith an assumed density of 0·2 (to obtain a precipitation element with a *mean* density f about 0·3).

As already explained, the coagulation element is regarded as a precipitation element n attaining a size such that :

a) during its growth it has settled a hundred metres or more relative to the air,
b) it has acquired a fall-speed exceeding 1 m/sec,
c) it can fall through several hundred metres of unsaturated air with insignificant shrinking.

If the evaporation of ice pellets of density 0·1 and 0·3 in falling through 300 m of nsaturated air is estimated as previously described for water droplets (I), Fig. 2 is btained. This shows that conditions (b) and (c) are fulfilled for radii of about 550 μ nd 300 μ respectively ; we now consider the rate of growth to these sizes and consider fterwards whether the condition (a) is also satisfied.

The growth of the ice particles is estimated from the equation

$$4\pi R^2 D\, dR = \{4\pi CkfR + \pi R^2 E_1 (V - v)\, m\}\, dt,$$

n which the first term on the right-hand side represents a mass added by condensation

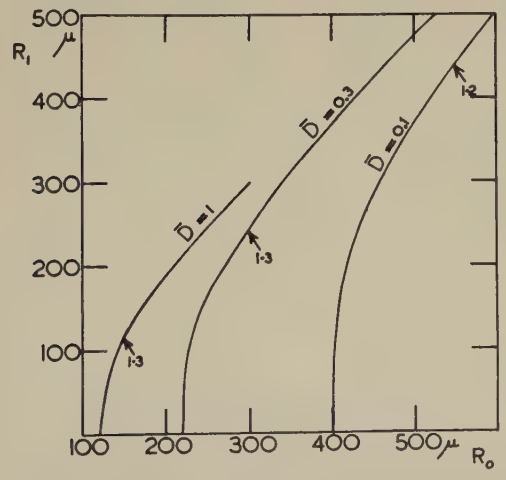

Figure 2. The radius R_1 of water droplets and of ice particles (of mean density $\bar{D}$, 0·1 and 0·3) of initial radius R_0, after falling 300 m through air of 90 per cent R.H. at a temperature of about $-10°$C. $R_1 > 0{\cdot}8R_0$ when $R_0 > 150$, 300 and 550 μ respectively (corresponding to fall speeds of 1·3, 1·3 and 1·2 m/sec), which radii are taken to define the size at which the particles become precipitation elements.

and the other a mass added by coagulation ; this reduces to

$$D\,dR/dt = Ckf/R + E_1\,(V - v)\,m/4 \qquad . \qquad . \qquad .$$

where D is the density of the added material ; R is the radius, V the fall-speed and E_1 the efficiency of catch of the particle ; m is the concentration of water in the form cloud droplets and v their fall-speed ; k is the coefficient of diffusion of water vapo in air, f the difference between the vapour density in the environment and that at ice surface, and C a coefficient taking into account the increased condensation due the motion through the air.

While the condensation is significant the Reynolds number is less than about and the value of C varies between 1 and about 2 (see, e.g., Kinzer and Gunn 195 f is less than f_1, the difference between the saturated vapour densities over liquid wa and over ice at the air temperature, because the ice surface is warmed by the heat condensation. It may readily be shown by use of the Clausius-Clapeyron equati that at temperatures between $-10°$ and $-20°$C, f_1/f varies between about 2 and 1 so that in our approximate calculations the quantity Cf in Eq. (1) may be replaced by or a constant, since at these temperatures f_1 varies little from an average value 0·20 g/m³. As previously described (I) E_1 may be obtained by modifying the formu of Langmuir, and values of k, m and v may be found for the same temperature regi in a series of model clouds having bases at 900 mb and base temperatures varyi between $-5°$ and 20°C. In most clouds the variations of k and m between lev where the temperature is $-10°$ and $-20°$C are small and may be neglected, b in the cold-base clouds the variation in m becomes considerable : in the $-5°$C-ba cloud its value increases from 0·8 to 1·7 g/m³. However, in view of the oth uncertainties we may still substitute into the equation an appropriate average val

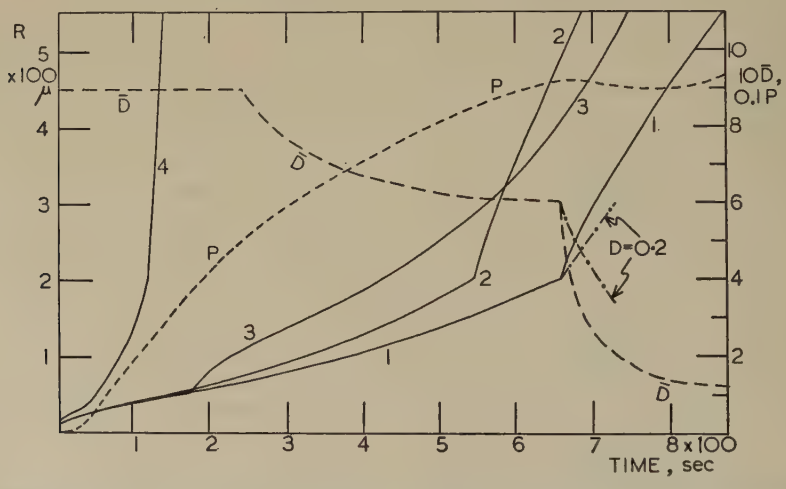

Figure 3. Growth of ice particles. Curve 1 : growth at about $-15°$C in cloud with base temperatu $-5°$C ; $k = 0{\cdot}25$ cm²/sec, $m = 1{\cdot}5$ g/m³, $r = 7\,\mu$. Rime density D assumed to be 0·6 wh $65\,\mu < R < 200\,\mu$ and 0·1 when $R > 200\,\mu$ or 0·2 when $R > 200\,\mu$ (pecked extension). The pecked li $\bar{D}$ shows the mean density, and the pecked line P shows the percentage of the added rime due to coagulati with droplets. Curve 2 shows the more rapid growth when $r = 10\,\mu$, and curve 3 the effect of assumi D to be 0·1 when $R > 50\,\mu$. Curve 4 shows growth in the cloud with base temperature 20°C ; $k = 0$ cm²/sec, $m = 6{\cdot}5$ g/m³, $r = 15\,\mu$ ($D = 0{\cdot}6$ when $35\,\mu < R < 200\,\mu$, $D = 0{\cdot}1$ when $R > 200\,\mu$: grow is so rapid in the latter stage that it is not significantly affected by a change in the value of D).

and Fig. 3 shows the solutions obtained numerically for clouds with base temperatures of 20°C and — 5°C, in which respectively $k = 0{\cdot}5,\ 0{\cdot}25\ \text{cm}^2/\text{sec}$; $m = 6{\cdot}5,\ 1{\cdot}5\ \text{g/m}^3$; and r, the average radius of the cloud droplets, = 15, 7 μ (these radii are determined from the values of m and the assumption that the droplet concentration is 10^6/g of air, i.e., about $10^3/\text{cm}^3$ at the cloud base).

In the cloud with a very high water content the distance fallen relative to the air during the growth of the elements is about 80 m, and it is nearly inversely proportional to m, increasing to about 500 m in the cloud with a cold base. In general, then, we may regard the radii chosen to define the precipitation elements to satisfy also the condition (a) quoted above.

Fig. 3 shows that even in the cold-base cloud of low liquid-water content the coagulation predominates over the condensation over much the greater part of the growth of the precipitation element. It appears that variations in the density of the added rime have little effect on the growth rate, because this is largely controlled by the term $E_1 (V - v)\, m/D$ (which is nearly equal to $E_1\, m\, V/D$) : because the geometry of the growth is such that the mean density of the particle closely follows that of the rime being added, V is about proportional to D and the important parameters are E_1 and m. This is illustrated by the result, included in Fig. 3, of assuming the added rime to have already a density of 0·1 when the particle radius exceeds 50 μ.

The efficiency of catch E_1 is largely determined by the cloud-droplet radius r. The abundance of condensation nuclei is such that the droplet concentration usually amounts to several hundred/cm³, and so when the water content m exceeds one or two g/m³ the average droplet radius is likely to be greater than 10 μ (and rather more than the 7 μ assumed above), and in a range where variations do not greatly effect E_1, as illustrated in Fig. 4. On the other hand this diagram suggests that when r is rather less than 10 μ a small change in its value affects E_1 considerably, while if r falls below some 7 μ, as is often likely in cold-base clouds or in the outer parts of warm-base clouds, then the growth of precipitation elements may be greatly retarded or even prevented because E_1 becomes very small or zero. To be more precise about this effect would require a better knowledge of the efficiency of catch at small Reynolds numbers than we now possess, but we may suspect that the small concentrations of nuclei found over oceans may allow showers to form in cold-base clouds noticeably more readily

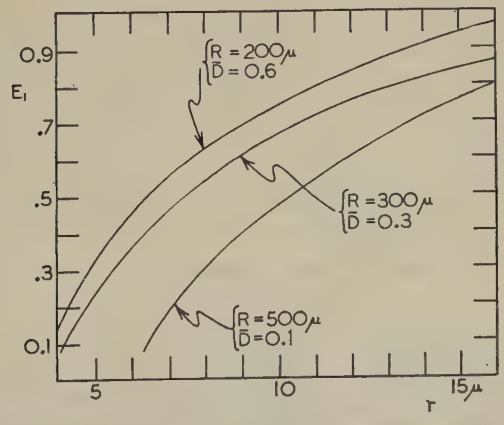

Figure 4. Estimated efficiency of catch, E_1, of spheres of various size and mean density falling through clouds of droplets of radius r.

in maritime air than well inland, where active nuclei are more plentiful, and wher consequently the average droplet radius may be smaller under otherwise simila conditions.

We may be confident, however, that in most situations it is the liquid-wate content m which is of first importance in determining the growth rate of coagulatio elements, and that the results shown in Fig. 3 fairly indicate the minimum perio which is required for the production of precipitation elements. In the dense clou with the warm base they are produced within about two minutes of the action of the ic nuclei. The tops of such a cloud therefore need rise only a few hundred metres abov the level at which a significant concentration of nuclei are active, for shower formatio to begin : showers are already likely when the summit temperatures fall below — 10°C On the other hand in the cloud with the very cold base the production of precipitatio elements requires over ten minutes, and must therefore begin deep inside the clouds ; the strength of the updraught there now influences considerably the summit leve at which shower formation occurs : with an average speed of only 3 m/sec the top must rise some 2 km above the level at which sufficient crystals appear, even thoug this be near the cloud base, so that even in favourable circumstances such a clou cannot be expected to produce a shower until it is at least 2 or 3 km deep and its summi temperature has fallen below — 20°C.

4. Observational evidence

So far few systematic attempts have been made to observe the summit-levels an temperatures associated with shower formation. Peppler (1940) analysed the results o German routine aircraft soundings made inland and concluded that shower formatio usually begins when the summit-level temperatures fall below — 12°C. Findeise (1942), from a small number of aircraft observations made over the North Atlantic inferred that over the ocean this critical summit temperature is raised by at least fou or five degrees. However, it seems that in these observations ' glaciation ' was ofte assumed from a fibrous structure at the cloud top, or even simply from the occurrenc of a shower, whereas in fact either appearance might be produced by the coalescenc process, which may readily cause shower formation in maritime cumulus whose summit barely reach above 0°C level (I). Probably for this reason Schwerdtfeger (1948 found — 3°C to be the most frequently reported summit-level temperature in over 25 observations of shower clouds made by aircraft (during the war) near 60°N 13°W The associated showers could hardly have been produced by ice crystals, which ar probably responsible for a secondary maximum frequency found at about — 13°C Fig. 5 summarizes an analysis of the British aircraft observations made over the Nort Atlantic during a period of more than two years, showing for particular ranges o cloud-base temperature the relative frequency with which cumulus and cumulonimbu (shower clouds) were reported, as a function of summit-level temperature. Apart fro the natural scatter on any general relation, it must also be obscured by observationa difficulties : some soundings were made in a poor light, or at such a distance from the tallest clouds that showers beneath them could not be seen, or such that the measure temperatures were not representative. Even so, there is a recognizable tendency fo the summit-temperature at which cumulonimbus is frequently reported to decreas with fall of cloud-base temperature, from about — 10°C at high base-temperatures t about — 20°C in the coldest clouds.

Recently Braham, Reynolds and Harrell (1951) have observed summer cumulus in New Mexico by double-theodolite and radar, and found that radar echoes never developed when the minimum summit-level temperature was higher than — 12°C ; at temperatures between about — 12° and — 24°C about a fifth (and at still lower temperatures a higher proportion) of the clouds produced echoes ; a few clouds gave no echoes even at temperatures below — 30°C. The cumulus in this region have high bases – often above 10,000 ft – and even in mid-summer the average base temperature is probably less than 10°C. The results of these writers are included in Fig. 5 and show a very similar trend to the other curves, although having a displacement which suggests that shower formation occurs less readily in New Mexico than over the North Atlantic.

Occasionally it may be observed from the ground that cumulus do not precipitate although almost certainly they contain ice crystals. For example at Dunstable on 19 Feb. 1948 there were prolonged falls of slight snow after dawn from broken stratocumulus at about 6,000 ft (— 12°C). Later the sun began to warm the ground and small cumulus appeared ; after a while their tops reached the level of the stratocumulus, which gradually dispersed. By mid-day there were numerous cumulus with bases at 3,000 ft (— 8°C) and tops reaching about 7,000 ft (— 14°C). Not until

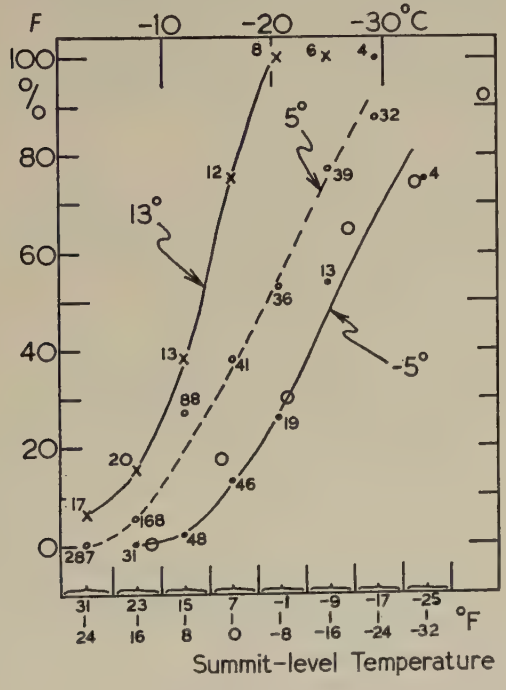

Figure 5. Percentage *F* of convection clouds reported as cumulonimbus, as a function of summit-level temperature, for base temperatures of 52-59°F (marked 13°C), 36-43°F (marked 5°C) and 20-27°F (marked — 5°C) : from soundings made over the North Atlantic by British meteorological flights during the period Jan. 1944 to Mar. 1946. The number of observations is noted beside each point in the diagram. The results of Braham *et al.* in New Mexico (average base temperature probably about 10°C) are shown by large circles.

mid-afternoon, when an aircraft reported the tops at 9,000 to 10,000 ft (— 20°C) did any of the cumulus become shower clouds, although the stratocumulus had earlier shown the atmosphere to contain many ice nuclei active at about — 12°C.

5. Conclusion

These observations certainly emphasize the great variation of summit-level temperatures at which shower formation begins. A general tendency for this to fall with decreasing cloud-base temperature can be discerned, but superimposed on this trend are variations due to a number of factors whose relations are illustrated schematically in Fig. 6.

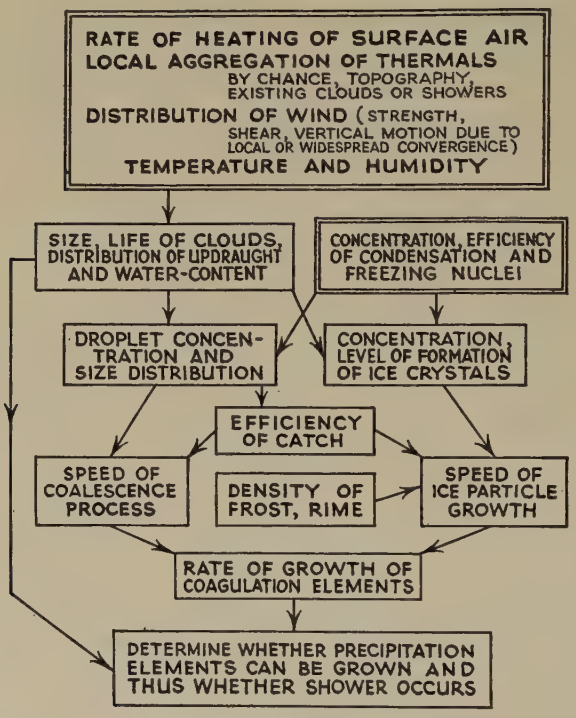
RATE OF HEATING OF SURFACE AIR
LOCAL AGGREGATION OF THERMALS
BY CHANCE, TOPOGRAPHY, EXISTING CLOUDS OR SHOWERS
DISTRIBUTION OF WIND (STRENGTH, SHEAR, VERTICAL MOTION DUE TO LOCAL OR WIDESPREAD CONVERGENCE)
TEMPERATURE AND HUMIDITY
SIZE, LIFE OF CLOUDS, DISTRIBUTION OF UPDRAUGHT AND WATER-CONTENT
CONCENTRATION, EFFICIENCY OF CONDENSATION AND FREEZING NUCLEI
DROPLET CONCENTRATION AND SIZE DISTRIBUTION
CONCENTRATION, LEVEL OF FORMATION OF ICE CRYSTALS
EFFICIENCY OF CATCH
SPEED OF COALESCENCE PROCESS
DENSITY OF FROST, RIME
SPEED OF ICE PARTICLE GROWTH
RATE OF GROWTH OF COAGULATION ELEMENTS
DETERMINE WHETHER PRECIPITATION ELEMENTS CAN BE GROWN AND THUS WHETHER SHOWER OCCURS

Figure 6. Relation of features determining whether shower formation occurs in cumulus clouds.

Shower formation happens rather suddenly at a particular stage in the aggregation of the cloud particles by microphysical events which occur inside every cloud. It cannot be understood by considering the small-scale physics alone : not only do the large-scale features of the cloud structure exert a controlling influence on the course of the microphysical processes, for example by modifying droplet size and concentration, but they possess the crucial importance of determining whether these processes continue to the stage at which the production of precipitation elements is accomplished and a shower becomes inevitable. Much remains to be learnt from both aspects before the the formation and intensity of shower rains can be understood, and the possibilities of their artificial control accurately assessed.

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Orographic cirrus clouds

By F. H. LUDLAM
Imperial College, London

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SUMMARY

Observations are described from which it is inferred that the air flowing over small hills (1,000 ft high may even in the cirrus levels suffer vertical displacements of 2,000 ft or more, which cause the formatio of clouds able to travel away from the hills, and may be responsible for a diurnal variation in cirru frequency.

1. INTRODUCTION

In discussing the irregular detail usually seen in systems of cirrus the writer ha suggested that the formation of these clouds is often affected by orographic disturbance reaching their level (Ludlam 1951). Because it is not generally recognized how ofte even small hills cause such disturbances some ground observations of cirrus formatio over hills about 1,000 ft high are here described, and supplemented with some aircra observations of similar clouds at high levels.

The lenticular clouds which form over or near hills are familiar to all observer Usually they occur in the lower or middle atmosphere and are composed of droplets s characteristically they have smooth surfaces and an extent about equal to or less tha that of the hills, and they remain almost stationary, but patches of altocumulus an stratocumulus of less regular shape are even more commonly formed near hills and the move away.

At levels where the temperature approaches — 40°C many ice crystals are likel to appear within the clouds, and their character may then change. This is becaus cloud usually does not form at ice-saturation, but only at or near saturation with respec to liquid water (see Ludlam 1948). Cloudless air at high levels may thus be super saturated with respect to ice ; if cloud forms in this air as it rises near a hill many of th supercooled droplets at first produced may freeze, and the crystals so formed continu to grow even when the air subsequently descends, because ice-supersaturation maintained unless the air sinks below its original level. Such a lenticular cloud woul have a sharply defined windward edge at the head of a long trail of fibrous ice clou which might extend over several hundreds of miles before its decay begins. Les extensive clouds may be formed if the humidity of the air is originally lower, whil irregularities in the humidity distribution, and also in those properties of the air and i flow which determine the character of its disturbance in crossing hills, may sometime be expected to result in the cloud formation being sporadic, so that the cirrus produce are patchy and irregular rather than marshalled into continuous banners lying downwin of the hills. After a while, especially in air passing over complicated ground feature the direct association between the cloud distribution and the topography become indistinct or totally obscured ; only when the clouds can be observed as they for is the association obvious.

Because of the great sparsity of active ice nuclei at high levels these extensiv transformations of lenticular clouds into cirrus are unlikely unless the cloud temperatur

is close to or below — 40°C, at which point all the cloud droplets are likely to freeze (see Mason and Ludlam 1951). This criterion, if obeyed, allows the minimum height of such clouds to be estimated, to within some two or three thousand feet, from routine upper-air soundings.

If the only nuclei present are solid insoluble particles then water saturation must be attained before a cloud can appear. It is possible that ice crystals can form before this stage is reached if there are present hygroscopic nuclei which become solution-droplets and freeze. However, unless they are associated with insoluble particles this freezing cannot occur at temperatures above — 40°C, which appears to have been established as the spontaneous freezing point of pure water or dilute solutions (see, e.g., Mason and Ludlam 1951). Moreover, it appears that this temperature of spontaneous freezing is reduced by increasing the concentration of the solution in much the same way as the ordinary freezing point is lowered (Shaw and Mason have supercooled saturated solution-droplets on a metal surface to temperatures below — 60°C, and are attempting to establish a relation between their concentration and freezing point). The sea-salt nuclei which may occur in the cirrus levels are likely to contain rather less salt than the average amount (about 10^{-13} g) in those found near the ground, and therefore even in rapid expansions have at any instant practically their equilibrium size under the existing humidity. It follows that at a relative humidity of about 75 per cent they exist as droplets of a saturated solution, whose spontaneous freezing point is likely to be about — 65°C, some 25°C lower than that of pure water. If we assume that the spontaneous freezing point and the ordinary freezing point of a salt solution are equally reduced at the same concentration, then we may relate the spontaneous freezing point of a solution-droplet to the relative humidity under which it is in equilibrium, as shown in Table I.

TABLE I.

Probable temperature of spontaneous freezing (°C)	Relative humidity (per cent)	Relative humidity corresponding to ice saturation (per cent)
— 40	100	68
— 50	90	62
— 60	80	57
— 70	70	54

It thus appears that even in the presence of the hygroscopic nuclei, ice crystals are unlikely to form until a considerable supersaturation over ice is reached, and that at temperatures somewhat below — 40°C a state of near-saturation over liquid water is required. When cloud forms over a hill at these temperatures the vertical displacement of the air can therefore be estimated as that required to increase the humidity from its initial value to saturation over liquid water.

The following sections describe three occasions when cirrus clouds were seen to form over hills in the south of England and Wales. The observations were made near Dunstable, at the northern end of the Chilterns (see Fig. 1).

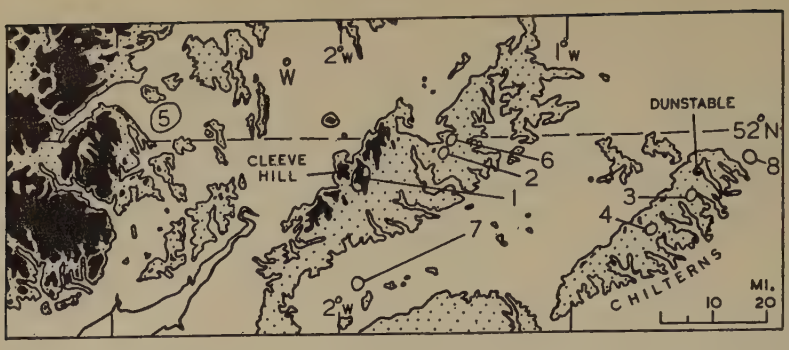

Figure 1. Map showing the principal regions of cirrus formation on 3 November 1951 (marked 1-4 winds WNW'ly), and on 1 December 1951 (marked 5-8 ; upper winds WSW'ly). W marks the position near Worcester, where an aircraft sounding was made on 1 December. Ground above 400 ft is stippled above 800 ft in black; above 2,000 ft and below 400 ft in white.

2. Cirrus formation, 3 November 1951

During the morning a depression over North Scotland was filling, and upper winds over England and Wales were becoming NW'ly as a cold pool over the British Isles receded before the advance of a warm upper ridge associated with a new frontal system approaching Ireland.

(*a*) *Observations*

About 09 GMT, thin lenticular clouds at about 2,500 ft were observed over the Chilterns ; they were very narrow and irregular, with constantly shifting detail. The only other clouds were some tenuous cirrus. The low clouds soon disappeared, but just before 10 GMT fractocumulus formed ; meanwhile it was noticed that a few miles to the south and south-west, and also in the far west, slender high-level lenticular clouds were continually appearing and becoming transformed gradually into diffuse and thin cirrus patches. The nearest were seen at an elevation of about 20° and appeared as small patches of very thin cloud, at first composed of rather fibrous flocciform cloudlets or small cirrocumulus flakes, and short transverse wavelets, which gradually spread and fused and became more fibrous as they moved away towards the south-east. The clouds forming in the far west were at a much lower elevation, and perhaps largely for this reason appeared dense and compact, with a smooth texture and creamy colour. Several thin clouds would grow and join into an irregular group, which after about 10 min became fibrous and diffuse, gradually becoming a more extensive but tenuous cirrus patch which then slowly evaporated as it drifted eastwards during a further period of rather more than an hour. Fresh lenticular formation occurred in about the same place at intervals of about half an hour. Fig. 2 shows some of the changes in the cloud distribution during a period of a little over an hour, and the uppermost picture of Plate V shows the general appearance of the clouds. The bearings of particular cloud details were observed carefully through a theodolite.

By 1130 GMT the cumulus had grown and increased and prevented continued observation of the high clouds ; it appeared that the generation of the high clouds declined with the growth of the cumulus, but until midday patches of cirrus cloud could occasionally be seen in about the previous positions.

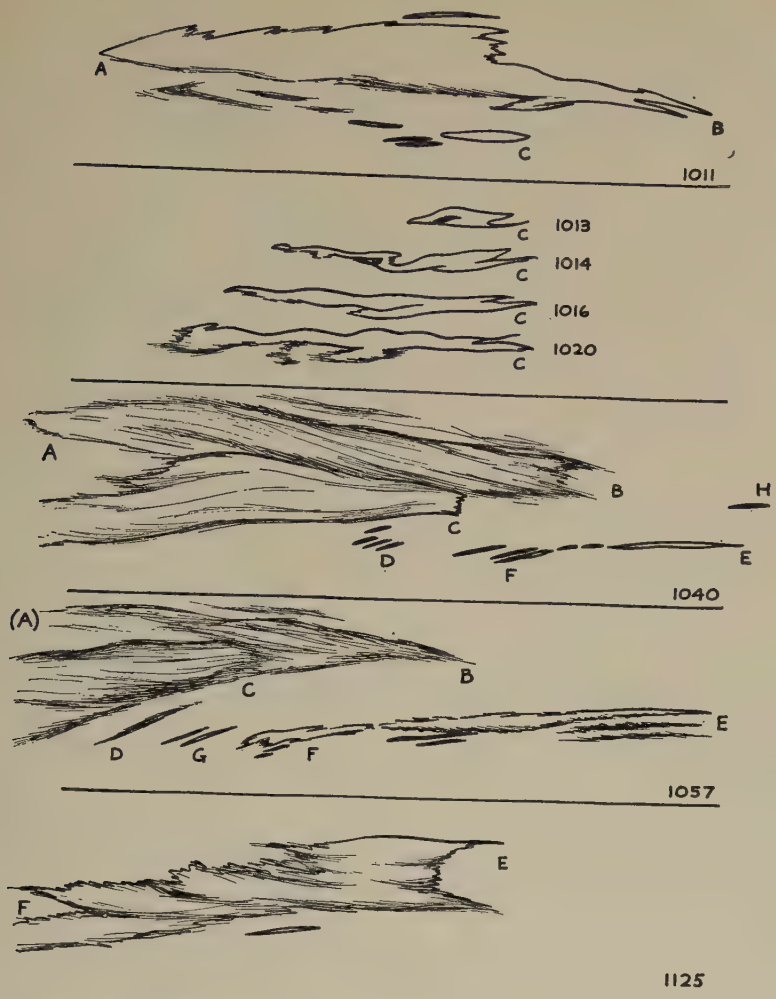

Figure 2. Changes in cloud details (identified by letters), 3 November 1951.

(*b*) *Discussion*

The winds at all levels up to 40,000 ft were WNW.'ly. The sounding made at 9 GMT at Larkhill, about 45 mi farther south, appears to be representative of the air n the region of lenticular cloud formation. The sounding is shown in Fig. 3.

The completeness of the transformation of the lenticular clouds into fibrous ice louds suggests that their temperature is likely to have been about — 40°C or less, ndicating a minimum height of about 20,000 ft. Assuming this (constant) height the heodolite observations of the displacement of details in the decaying clouds give stimates of 283°, 28 kt and 285°, 24 kt for the wind at their level, in satisfactory agree- ient with the wind reported with the Larkhill sounding at 20,000 ft (450 mb). In ne or two places a faint fallstreifen trail lying back towards the north-west showed the louds to be in the region, above the 500 mb level, where the wind veers with height. f, moreover, the positions of the regions in which the lenticular clouds were forming re plotted on the assumption that their height was just over 20,000 ft, then they are ound over or just to leeward of the ridges of the Cotswolds and the Chilterns (Fig. 1).

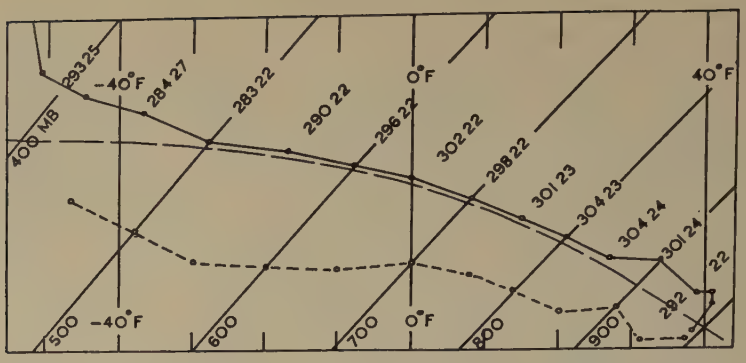

Figure 3. Sounding at Larkhill, 0900 GMT, 3 November 1951. Winds (in degrees and knots) are ente on the appropriate isobars of the tephigram. Dew points lie on the pecked line.

In view of all this evidence the error in the assumed height is unlikely to exceed abo 3,000 ft and the clouds can certainly be associated with local disturbances due to the hills. The very slow dissipation of the ice clouds suggests, if the cloud air returned about its original level after passing over the hills, that the humidity was origina not much below that representing ice saturation. Since at a temperature of abo — 40°C the difference between the frost point and the dew point is 3·5°C, the verti (adiabatic) displacement required to produce cloud was probably rather more th 1,200 ft. If the humidities recorded on the Larkhill sounding are accepted, a displa ment of about 2,000 ft is indicated.

Thus in an air stream of common characteristics the air at 20,000 ft experience vertical displacement of between 1,000 and 2,000 ft in passing over ridges betwe 800 and 1,000 ft high (if the displacement occurred over a horizontal distance of abo 5 mi the mean upward speed was 2 ft/sec to 3 ft/sec). As a result the sky contain considerable amounts of cirrus cloud even though the air was generally not saturat with respect to ice.

3. Cirrus formation, 30 November 1951

A broad warm sector covered the country and winds up to cirrus levels we between W. and WNW.

(*a*) *Observations*

During the morning cirrus clouds very similar to those already described form over the Cotswolds and Chilterns (Plate V, centre). On this occasion the evaporati of the individual clouds was more rapid, but they were more quickly replaced by fre formations, so that large patches of cirrus persisted in the region of formation. The were no other clouds in the sky. The angular elevation of the clouds forming over t Cotswolds was a little greater than on the previous occasion, suggesting that the clou were at a greater height. Detailed observations were not made after 09 GMT.

(*b*) *Discussion*

The sounding made at Larkhill at 09 GMT is shown on Fig. 4 which also contai the frost points observed there in the afternoon by an aircraft of the Meteorologi Research Flight ; the temperature soundings were very similar to those made

Plate V. The uppermost photograph shows the cirrus clouds over the Cotswolds (about 60 mi away) at 1030 GMT, 3 November 1951. In the top left-hand corner is a small cloud which has just formed over the Chilterns. The centre picture shows cirrus forming over the Cotswolds at 0905 GMT, 30 November 1951. The two white triangles show the positions in the sky of the principal nearby regions of cirrus formation on 3 November (left-hand triangle) and 1 December 1951 (right-hand triangle). The lowest photograph shows the appearance at 1035 GMT, 1 December, of the cirrus bands in the lee of the Cotswolds and (lower in the picture) in the lee of the Black Mountains (about 100 mi away).

Plate VI. Cirrus bands in the SW. sky, 1040 GMT, 1 December 1951.

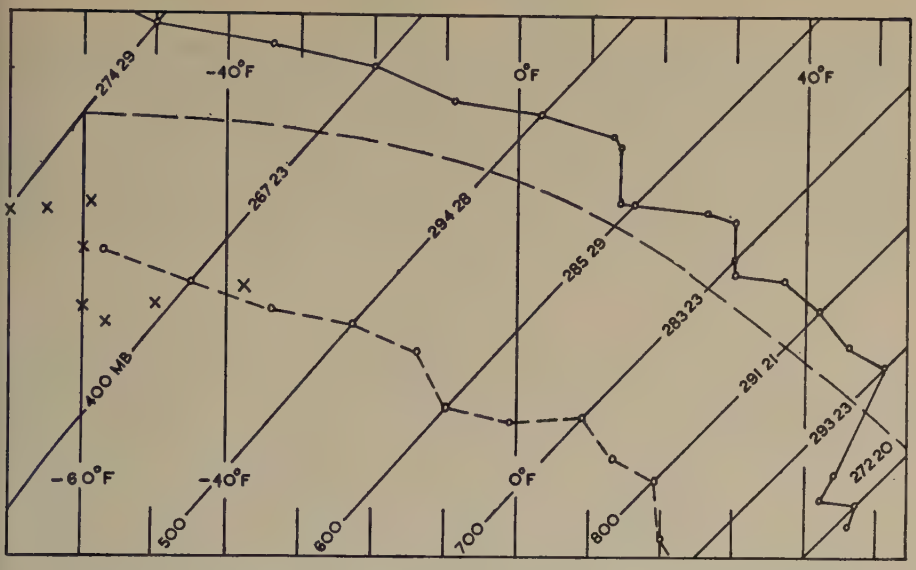

Figure 4. Sounding at Larkhill, 0900 GMT, 30 November 1951, and frost points observed there in the afternoon by the Meteorological Research Flight (crosses).

Hemsby and Liverpool. If it is assumed that the clouds near the Cotswolds were vertically above the hills their angular elevation indicates their height to be about 28,000 ft, which is supported by the temperature of about — 40°C recorded over Larkhill at this level, and perhaps also by the occurrence there of rather higher frost points. The rapid evaporation of the individual clouds is also consistent with the low humidities reported, which suggest that a vertical displacement of between 4,000 and 5,000 ft was required to produce the clouds. This is more than twice that estimated on the previous occasion ; the increase may be associated with the comparative shallowness of the layer of unstable air near the ground.

4. Cirrus formation, 1 December 1951

A cold front moved south-east across England and Wales overnight and during the day became quasi-stationary over the English Channel. In the warm air aloft over England and Wales the wind remained WSW.'ly.

(*a*) *Observations*

Throughout the day from soon after sunrise the sky contained several long parallel bands of cirrus, but no other cloud. During the morning these bands were found to be lying in almost straight lines along 247°. In the southern part of the sky the bands were broad and extended from horizon to horizon ; they contained multitudes of transverse fibres or waves, and some faint fallstreifen (Plate VI). Nearer the zenith and in the northern sky the bands were narrower and widely separated ; their western ends were sharply defined points a few degrees above the horizon (Plate V, lowermost picture), but to the east the bands extended at least as far as the east coast of England. The westernmost points of the bands remained almost stationary, although their nearer details could be seen moving rapidly along the band length, so that evidently the clouds

were being formed almost continuously in particular places associated with certa ground features. During a period of at least four hours after 10 GMT the point of t most northerly band clearly visible remained in the same place, but about 1045 G the formation of the nearer band shown on Plate V ceased, and its westernmost e then moved steadily into the eastern sky, so that by soon after noon the band h altogether disappeared. At the same time much of the cirrus in the southern s dispersed, but the northernmost band persisted throughout the afternoon, movi slowly south as the upper wind veered slightly.

(*b*) *Discussion*

During the early afternoon an aircraft sounding was made near Worcester a reported the cirrus tops to be at about 23,500 ft (400 mb, — 35°C). However, t motion and orientation of the clouds observed in the morning suggests that th were then some 2,000 ft higher. Assuming this greater height the regions in which cirr bands were forming are found to lie in the positions marked 5, 6 and 7 on Fig. These places are respectively about 15 mi to 20 mi in the lee of the Black Mountai (2,660 ft), the northern Cotswolds near Cleeve Hill (1,070 ft), and the southern Cotswold About midday another band was observed to be forming about the same distan behind the northern Chilterns (position 8). The broader cloud-bands farther sou were continuous as far south-west as could be seen, but may have been forming ov the Mendips (1,070 ft) and Exmoor (1,700 ft), in SW. England. The soundings ma at 09 GMT at Larkhill and Liverpool are shown in Fig. 5. The humidities record show that at the cloud level a lift of between 2,000 and 3,000 ft was required to satura the air with respect to liquid water. A similar fall in the level of the cloud tops in t neighbourhood of the place where they formed might be expected, but was n noticeably shown by the appearance of the clouds.

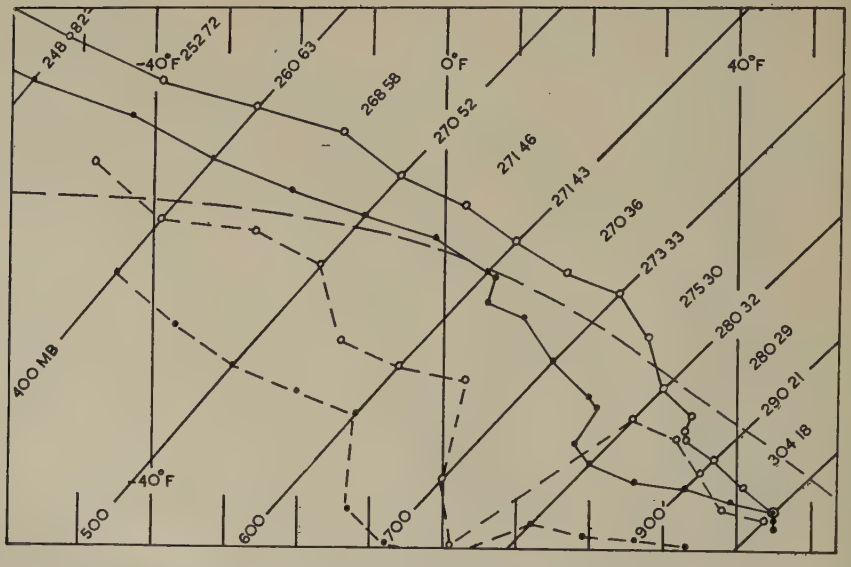

Figure 5. Soundings at Larkhill (open circles) and at Liverpool (dots), 09 GMT, 1 December 1951. T winds entered on the diagram are those observed at Larkhill.

5. Aircraft observations

Glider pilots in this country are becoming increasingly aware that the lenticular nd wave clouds which indicate orographic disturbances in the wind flow are very ommon, and are not restricted to the neighbourhood of large mountains or to the wer atmosphere. At high levels probably not all of these clouds are associated with ne vertical speeds, exceeding 1 m/sec, which are useful to the glider pilot, and their xploration by gliders has not yet been attempted. The experience of the pilots, however, as allowed them to recognize and examine high-level wave clouds during flights in owered aircraft.

Flt.-Lt. Plenderleith has given me a detailed account of a flight on 10 Dec. 1951 n a Prentice aircraft, in which he found wave motions at 9,000 ft over the northern Cotswolds with upwards speeds of about 3 m/sec. Some lenticular clouds were found t 7,500 ft in the north-westerly air stream, whose speed increased from about 20 kt at ,000 ft to about 35 kt at 9,000 ft.

Austin (1952) has made detailed observations over southern England on 24 Sept. 951 of wave clouds, at a series of heights up to 32,000 ft, which were produced by idges less than 1,000 ft high. A vertical speed of about $1\frac{1}{2}$ m/sec was estimated ear a lenticular cloud at 22,000 ft. Mallett (1951) has described a flight near lenticular louds at 20,000 ft in the lee of the Pennines on 12 Jan. 1951 ; up and downdraughts eside these clouds were estimated to reach 10 m/sec.

6. The diurnal variation of cirrus clouds

It often seems that cirrus clouds appear preferentially about sunrise and sunset nd are much less frequent about midday. This is particularly true of well-defined louds which have only recently formed : much of the isolated cirrus seen about nidday has a decayed appearance suggesting that it is several hours old. This diurnal ariation of cirrus clouds is quite marked in the hourly observations made over several ears by Spencer Russell (1913) at Epsom, and cannot be explained solely by the very avourable conditions of illumination under which high clouds are seen when the sun s near the horizon, or by the increase in low-level convection clouds over land during he middle of the day, because these are usually well broken and do not hide higher louds from an attentive observer.

Scorer (1952) has concluded that the wave disturbances in air flowing over hills re likely to be greatly reduced when the air near the ground is unstable, so that the rographic formation of cirrus is in general less likely during the day-time for this eason. Occasionally it may be observed that the leading edge of frontal systems of irrus advances very rapidly towards sunset. For example on 8 Jan. 1948 the leading dge of a cirrus system, associated with a depression approaching at about 35 kt, was bserved from Dunstable to advance suddenly from just above the western horizon a ittle before sunset to an elevation of 6° an hour later, corresponding to a speed of about 0 kt. This rapid advance was almost certainly due to fresh formation of cirrus near he Welsh mountains ; although the cloud was later seen to consist of separate strips, t these low elevations it appeared continuous with a clearly defined edge. On other ccasions thin frontal cirrus found to be covering much of the sky at dawn ubsequently advances and thickens very slowly, or may even recede or disperse luring the morning, although the associated depressions or fronts appear to continue noving steadily.

7. Conclusions

Several observations have been quoted which show that ridges or hills only abo 1,000 ft high can disturb the air flow considerably at levels up to least 30,000 ft. T air at high levels may be displaced vertically 2,000 ft or more at upward speeds exceedi 1 m/sec. Indications of such disturbances can often be found in systems of cirr clouds in air flows of common characteristics ; they presumably also often occur witho attendant cloud formation, and may be responsible for other phenomena, for examp the local formation or persistence of aircraft condensation trails, and spurious inversio or superadiabatic lapse rates in balloon soundings (see Austin 1952, Appendix I

At temperatures of about — 40°C or less the droplets of clouds formed in the disturbances readily freeze, and the ice clouds then produced can move away fr the hills. Sometimes large amounts of slowly-decaying cirrus may thus be produc in air which is generally barely saturated, or even unsaturated, with respect to i The formation of orographic cirrus is less likely during the middle of the day when t air near the ground is often unstable, and consequently cirrus is observed less frequen inland at this time than about sunrise and sunset.

Acknowledgement

I am indebted to the Director of the Meteorological Office for permission include in Fig. 4 the observations of the Meteorological Research Flight.

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Observations of the westerlies over the sea

By P. A. SHEPPARD, H. CHARNOCK* and J. R. D. FRANCIS
Imperial College, London

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SUMMARY

Observations of wind speed and direction at 50 m intervals of height in the first few hundred metres ave been made over the NE. Atlantic during 10 winter days of westerly winds. The following results ere obtained :

(i) The vertical gradient of wind speed is of the same order at all levels and there is no identifiable ransition from a layer of frictional influence to a layer of friction-free wind.

(ii) The wind speed at 300 m was on the average 21 per cent greater than the surface wind, though n 24 per cent of cases the latter was greater than the former. The mean change of direction in the same nterval of height was 3·6° veer but the spread was much greater than the mean. On over 30 per cent f cases the wind at 300 m was backed on the surface wind and the speed at the upper level was then nly 5 per cent greater than the surface speed.

(iii) The vector variation of wind velocity with height in the layer 200-500 m is accountable as nermal wind.

(iv) Turbulence of scale greater than about 10 min is of order 10 per cent of the mean motion and ncreases on average with height.

(v) The mean inclination of the surface wind to the isobar is not more than 1° or 2° and is more kely to be towards high pressure than low.

It is inferred that the classical conception of the frictional boundary layer in a barotropic atmosphere as little validity in the atmosphere over the sea. In particular the shearing stress in the first few hundred netres of the westerlies probably varies only slightly with height and may increase if the thermal wind strong. Measurement of surface drag by the method of frictional geostrophic departure is hardly ossible in the baroclinic westerlies.

1. INTRODUCTION

This paper describes detailed pilot-balloon observations which were undertaken n order to determine the aerodynamic drag of the sea surface. The work failed in s primary purpose for reasons which will appear but has brought to light a number of eatures of the behaviour of wind in the first few hundred metres over the sea which re thought to be significant.

The aerodynamic drag of the sea surface is of considerable climatological, oceano- raphic and synoptic importance yet much uncertainty exists as to its numerical nagnitude in a given surface wind, u_0, and as to the manner of its variations with u_0. Thus on the one extreme Neumann (1948) considers the surface drag per unit area $_0$ to vary as $u_0^{3/2}$, on the other extreme Durst (1949) and Francis (1951) advance vidence for a u_0^3 relation, while Munk suggests that there are two u_0^2 regimes with discontinuity in the factor of proportionality at $u_0 \simeq 6$ m/s. This uncertainty exists artly because some assumption is involved in whatever method is chosen for measuring $_0$, and partly because of the difficulty in obtaining sufficiently accurate values of the nean wind velocity and other quantities from which τ_0 may be deduced. It was this tate of affairs that led us to make an expedition for the measurement of wind in detail nd with greater than usual accuracy in the first few hundred metres over the sea n order to deduce the drag from the ageostrophic motion due to friction in this layer, ommonly called the terrestrial boundary layer.

* Also of the National Institute of Oceanography, Teddington

2. The site and the measurements

The observations reported here were made over the eastern North Atlantic, usi
the Scilly Islands (49°55′N, 6°20′W) as base, during seven days of the period 3–
January 1951, the weather being generally of a westerly type with a rapid successi
of troughs and ridges moving eastward over the station.

The Scillies (Fig. 1) are a group of small low-lying islands about 45 km to WS
of Land's End, England, the largest islands being a few km² in area and a few k
apart. The average height of land is about 20 m, with local prominences up to abo
50 m. The islands provide an admirable base from which theodolite observations m
be made of balloons travelling over the sea, in air which has not been influenced
land for at least 200 km upwind with winds between SSE. through W. to N. and f
many times that distance over most of this range of direction.

Pilot balloons, inflated to give a low rate of ascent of about 1 to 1½ m/sec, we
released, whenever the state of sea permitted, from a small boat lying a few km
windward of the main islands. The balloons were tracked by three theodolites set
at appropriate surveyed points a few km apart on the islands, synchronised readin
being taken at ½-min intervals over a period of 10 min or so (600 m or more in heigh
after which the balloons were abandoned. Synchronisation was effected by visu
signalling, aided in advance as necessary by wireless communication. Ascents we
made at 10 to 20-min intervals over those hours of each day in which observatio
were possible. On some days the state of sea made open-boat releases hazardo
and a suitable land-release point, e.g., a low-lying spit, such as D (Fig. 1), was th

Figure 1. Outline map of Scilly Isles, showing 100 ft and 150 ft contours (dotted lines). P is a typic
release point, and A, B, C, D typical theodolite positions.

chosen with due regard to wind direction and suitable theodolite sites. Whether open-boat or land, the release points chosen ensured that the balloons travelled wholly or mainly over the sea throughout the period of observation. In no case did the air into which the balloon was released pass over land for more than a few metres upwind of the point of release. It is possible that the ascents Nos. 111 to 134 made on the last two days of observations (release point E, Fig. 1) were slightly influenced by rising land to the left of the trajectory in the first half minute or so of flight.

Of 134 ascents 103 were analysed.

3. Working-up of observations

Plan trajectories were plotted for all ascents on a scale of 1 cm = 53·7 m. The triple-theodolite readings enabled a balloon to be located within a sphere of about 3 m radius, so that the error in wind speed over a ½-min interval was then about ± 0·15 m/sec and in wind direction about ± 1° in a wind of 10 m/sec. Occasionally the readings from two theodolites only were available for parts of some ascents, but the earlier and later triple-theodolite fixes enabled accuracy in displacement to be reasonably maintained.

A sample test having shown that the rate of ascent of a balloon was sufficiently uniform on a given flight, the height of the balloon at the end of each flight was determined and the mean rate of ascent found for the flight. Each plan-trajectory was then marked at the points where the balloon passed successively through the 50, 100, 150, etc., m levels.

The ' surface ' wind was taken from the first ½ min of flight and ascribed to a level of 20 m which is sufficiently near the mean level, 22 m, for the first ½ min of flight Wind speed and direction at the levels, 50, 100, 150, etc., m were obtained from the ½-min displacements containing the respective levels or, where one of these levels nearly or quite coincided with a level of fix, from the mean of the two ½-min displacements preceding and following the reference level. The mean rate of ascent for all flights being 1·44 m/sec, the wind velocities extracted refer on average to means over 43 m-thick layers. Wind speed was measured to the nearest 0·18 m/sec (1 mm on plot for ½-min displacement) and wind direction to the nearest 1°.

4. Broad survey of results

Heights. The heights attained on the flights analysed are distributed as follows :

Height (m)	200 < 300	300 < 400	400 < 500	500 < 600	600 < 700	700 < 800	800 < 900	> 900
Number	10	13	17	23	24	11	5	3

the mean being 555 m. The spread leads to some inhomogeneity in the data which will be apparent later.

Vertical velocity of balloons. The distribution of rates of ascent of balloons, using the mean for each flight, is :

Rate of ascent (m/sec)	0·8 < 1·0	1·0 < 1·2	1·2 < 1·4	1·4 < 1·6	1·6 < 1·8	1·8 < 2·0	> 2·0
Number	12	18	22	25	23	11	4

giving the mean 1·44 m/sec. The spread is not large and is not considered likely

to falsify the analysis of wind. Thus even the few relatively high rates of ascent c about 2·0 m/sec give ½-min vertical displacements only slightly larger than the 50 r intervals of height for which winds have been extracted.

Wind speed and direction. The complete results of wind speed and direction a the surface and thereafter at 50 m intervals to the top of each flight together with se and air temperatures are given in the Appendix. A complete evaluation of vertica and horizontal displacements for every ½ minute of flight for all flights is availabl on request.

The distribution of surface-wind speeds is :

Surface-wind speed (m/sec)								
	< 5·2	5·3 7·0	7·1 8·8	8·9 10·6	10·7 12·3	12·4 14·1	14·2 15·9	16 17
Number	15	19	20	20	16	9	3	1

the mean being 8·8 m/sec. The range is reasonably representative of mean Januar conditions. (The long-term mean wind speed for January is 9·8 m/sec (M.O. 446b)).)

The surface direction varied between 216° and 336°, and was about as likely t be north of west as south of west.

5. Profiles of mean wind speed

Profiles of mean wind speed have been prepared from ascents grouped accordin to the surface-wind speed. They are shown in Fig. 2, together with a selected singl wind profile (Run 51) representative of the accuracy attained on a single flight.

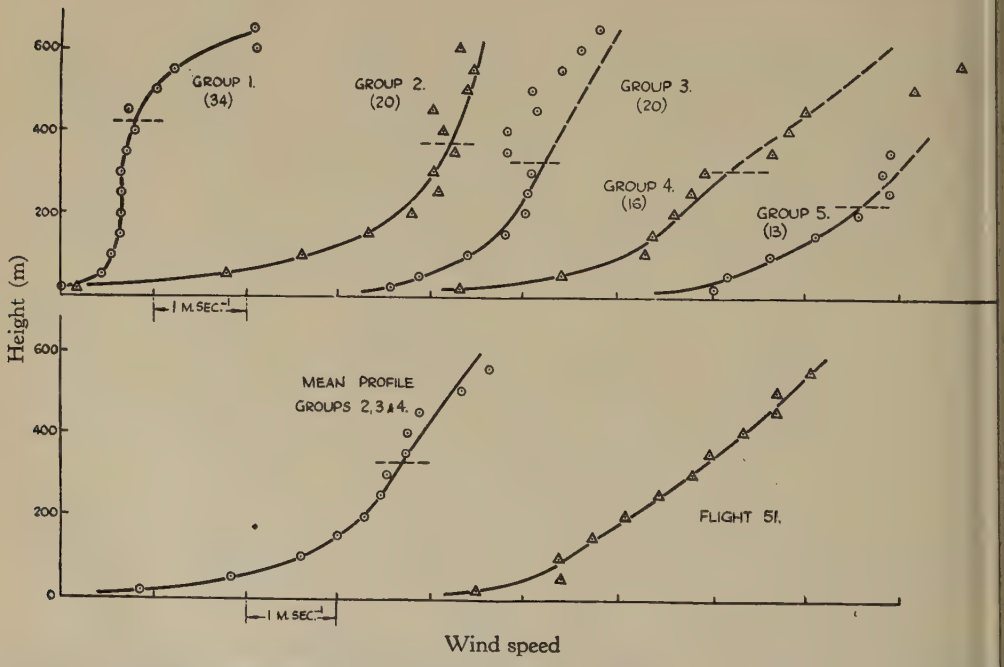

Figure 2. Profiles of mean wind speed grouped according to surface-wind speed : (1) $\bar{u}_0$ = 5·3 m/sec (2) $\bar{u}_0$ = 8·1 m/sec, (3) $\bar{u}_0$ = 9·8 m/sec, (4) $\bar{u}_0$ = 11·4 m/sec, (5) $\bar{u}_0$ = 14·1 m/sec. Also mean of Group 2, 3 and 4 and results of an individual run (Run 51).
Data inhomogeneous above dashed lines. Number of ascents forming means at lower levels shown against curves.

It is noticeable that there is little tendency for the gradient of wind speed to diminish appreciably with height above about 100 m. Certainly there is no tendency to constancy of wind speed with increasing height such as is implied by the classical theory of the friction-boundary layer. The curve for Group 1 does indeed show rather constant speed following the near-surface rapid increase but at higher levels the increase is again very marked. It is unfortunate that inhomogeneity of data obscures the variation at upper levels to some extent but the mean for Groups 2, 3 and 4 leaves little room for doubt about the reality of the sustained increase throughout the layer examined. In this respect the single profile, Run 51, shows the behaviour clearly and more strongly.

In a baroclinic atmosphere with horizontal temperature gradient more or less normal to the surface wind, the wind speed, if quasi-geostrophic, will show a sustained increase with height. But it has perhaps been tacitly assumed that the magnitude of the thermal wind over a few hundred metres is generally much smaller than the frictionally imposed gradient over the same depth near the surface. The curves of Fig. 2 suggest that this may not be true and it would be desirable therefore to determine the thermal wind over the layer examined on the occasions to which the curves refer. This has not proved to be possible owing to the difficulty in obtaining representative surface temperatures in the area, but a test of almost equal validity is to compute the thermal gradient necessary, on the geostrophic assumption, to provide the observed gradients of wind speed, above 200 m say, for the given curves. In the latitude of Scilly the required relation is :

$$\frac{\partial T}{\partial y} = -0{\cdot}324\,\frac{\partial u}{\partial z}\ (^{\circ}\mathrm{C}/100\ \mathrm{km}),$$

where $\partial u/\partial z$ is in m sec^{-1} km^{-1}. We then find for the layer 200 m-500 m (neglecting change of direction with height – see following section) :

$$\text{Mean of Groups 2-4} \qquad -\frac{\partial T}{\partial y} = 1{\cdot}2^{\circ}\mathrm{C}/100\ \mathrm{km}$$

$$\text{Run 51} \qquad -\frac{\partial T}{\partial y} = 1{\cdot}9^{\circ}\mathrm{C}/100\ \mathrm{km}$$

the y axis being roughly towards north since the winds were westerly.

These values are certainly of the right order for a baroclinically active region such as the north-east Atlantic was during these observations, and is generally. Thus the mean horizontal gradient of surface-air temperature in the Scilly Islands region in January is (M.O.483, 1948) 1·0°C/100 km, directed to E. of N. Moreover strong supporting evidence that the observed wind gradient above 100 m or so was primarily of thermal origin is to be obtained from the mean gradient of westerly component of wind between 900 mb and 700 mb at the Camborne radar-wind station, 50 km to east of Scilly. On the same days this gradient was the same within ten per cent as in the air over Scilly between 200 m and 500 m.

The exceptional profile shown by the Group 1 soundings is roughly explicable in the same terms. This profile derives largely from observations made on 10 Jan. 1951, during the morning of which the wind direction was between 315° and 336°. During this period the *wind backed with height from the surface* and Camborne showed a strong westerly thermal between 900 mb and 700 mb. It is not surprising therefore that there

is little increase of wind speed with height for some distance above the initial friction increase, since wind and thermal wind are more nearly normal than parallel.

We conclude from this analysis that the effect of friction on wind speed ov the sea is only clearly discernible in the first 100 m or so and that the speed variatic at higher levels is largely due to horizontal temperature gradient.

6. The variation of wind direction with height and its relation to th gradient of speed

Fig. 3 shows the variation of wind direction with height by means of frequenc diagrams for differences of the wind direction at a number of heights from the directic at 200 m. Mean differences are also shown which amount to about 1° veer per 100-f increase in height.

The most striking feature of the diagram is the almost symmetrical spread c either side of 0° for heights up to 400 m. Westwater (1943) has presented frequenc curves for the SE. Trades, the Atlantic and the Mediterranean, over a deeper laye (1000 m) which show much the same form. Some of the spread is undoubtedly du to turbulence of a scale of about the height intervals, but much is also due to systemat backing as well as veering with height on individual flights. Reference has alread been made to backing with height on one day when the thermal wind was backe appreciably on the actual wind but there were also other occasions when th occurred.

The change of direction with height has an interesting relation with the chang of wind speed with height, which is shown for a sequence of flights on 8 Jan. in Fig. 4 Two features are to be noted : (i) a measure of correlation between the directio and speed-ratio changes, (ii) a marked tendency for the wind speed at the upper lev to be less than the surface wind when the latter is veered on the former. The realit of these features is shown by an analysis of all the flights, as given in Table I.

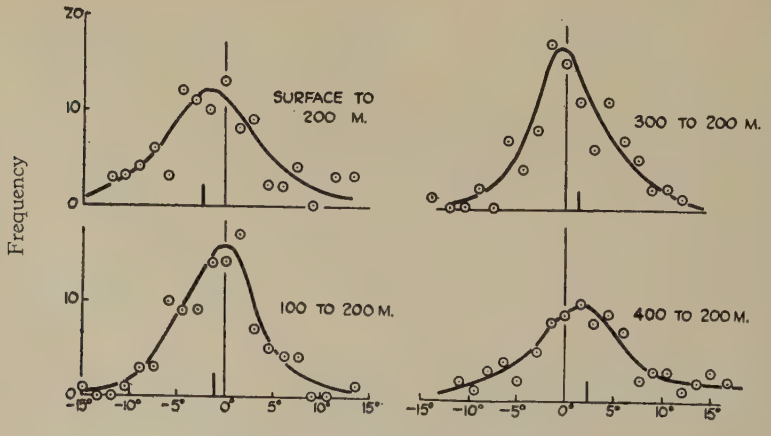

Figure 3. Frequency distribution of variation of wind direction with height using the 200-m win direction as reference. Positive direction implies veer relative to 200-m direction. Mean values c direction at each level relative to 200-m direction shown by short vertical lines on abscissae.

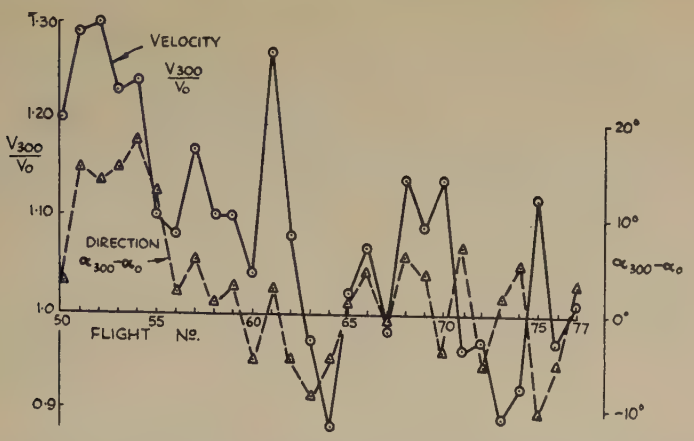

Figure 4. Curves of speed ratio V_{300}/V_0 (subscripts refer to heights in m) and direction difference $(\alpha_{300} - \alpha_0)$ against time for a sequence of flights on 8 Jan. 1951, at approximately 10-min intervals. Flights are shown uniformly spaced.

TABLE I. Relation of vertical gradient of wind speed to change of wind direction with height, for layer 0-300 m

V_{300}/V_0 = ratio of 300 m wind speed to ' surface '-wind speed.
$\alpha_{300} - \alpha_0$ = difference of direction of wind at 300 m and surface.

	Means when $V_{300}/V_0 \leqslant 1{\cdot}0$	Means when $(\alpha_{300} - \alpha_0)$ negative	Overall means
V_{300}/V_0	0·94 (24 cases)	1·05 (33 cases)	1·21 (103 cases)
$\alpha_{300} - \alpha_0$	− 1·2°	− 5·4°	+ 3·6°

Note that over 30 per cent of the flights show the wind at 300 m backed on the surface wind and nearly a quarter give the wind speed at 300 m less than or equal to the surface speed.

The interval between the measurement of the surface wind and that at 300 m is comparable with the interval between soundings. However, the occasional apparent persistence of both high and low (or reversed) gradients over several successive soundings suggests that the curves of Fig. 4 are evidence of partly advective changes in the quantities displayed. The inference then is that the air may possess widely different speed and direction profiles in intervals of about 10 min and upwards in time or of about 5 km and upwards in horizontal extent as a result of large-scale eddying motions. There appears to be no significant correlation of the profile changes such as are shown in Fig. 4 with the surface-wind direction itself or with the temperature difference between sea and air, but there is a rough inverse relation between the ordinates of Fig. 4 and the surface-wind speed. Further properties of the large-scale eddying motions will be presented in Section 10 and we note here only that such motions, capable of reversing the mean gradients of speed and direction, are likely to be important in determining the pattern of momentum transfer and, therefore, the properties of the terrestrial boundary layer.

The climatological veer of the wind with height (northern hemisphere) in the first few hundred metres, present also as a mean feature in the observations now

under discussion, is commonly attributed to the effect of friction – precisely, to th second differential of shear stress with respect to height. This is not necessarily th correct interpretation. Thus we have already seen that the increase of wind spee with height above the 100 m or so thick layer next the surface is reasonably interprete as due to horizontal temperature gradient and geostrophic control. Similarly th observed veer of wind is also of the order to be expected from the conjunction c surface-wind velocity and thermal field. For the layer 200 m-500 m we find th following horizontal vector temperature gradients $\nabla_h T$ if we assume the vector win change is wholly due to the latter :

Mean of Groups 2-4	$\nabla_h T = 1{\cdot}4°\text{C}/100$ km at 205°	Direction toward
Run 51	$\nabla_h T = 2{\cdot}9°\text{C}/100$ km at 210°	higher temperature

These values are necessarily somewhat higher than those found in Section 6, whic were components normal to the wind, but they remain of the right order and the mea of Groups 2-4 is only 40 per cent greater than the climatological mean and is in practicall the same direction. The individual Run 51, a rather extreme case of speed and directio change with height, is also not unreasonable by virtue of the passage of an occlusion shown on the *Daily Weather Report*, two or three hours prior to this run. Not ever single run would of course give such a reasonable value on account of the presume effects of larger-scale turbulence, discussed above. But Run 51 is by no mean exceptional and the smooth variation of wind speed and direction for this run suggest that it was representative of the air mass in which it was taken.

It must not be inferred that the *mean* variation of wind direction with heigh over the sea is *only* to be interpreted in terms of thermal wind. But we consider tha a case has been made for ascribing a material part of the variation to this cause. Moreove it is reasonably consistent with the observed variation in other waters, as found fo example in the kite observations from *S.S. Meteor* in the SE. Trades and Westerlie of the South Atlantic (Kuhlbrodt and Reger 1933). The analysis shown in Table I has been made in which again the vector temperature gradients have been compute on the assumption that they are solely responsible for the change in wind in the layer referred to, and rather ill-defined climatological averages (M.O. 483) are shown fo comparison. No value of $\nabla_h T$ is given for the layer 200-500 m in the SE. Trade since the occurrence of a wind-speed maximum in the neighbourhood of 200 m make computation quite uncertain. The agreement between calculated and observe temperature gradients is good, except for direction in the SE. Trades, in which, accordin to the surface air-temperature isotherms in M.O.483, the direction is quite variabl with position.

TABLE II. WIND SPEED AND DIRECTION PROFILES IN S. ATLANTIC FROM *S.S. Meteor*, WITH INFERRE AND OBSERVED HORIZONTAL TEMPERATURE GRADIENTS

	Wind (m/sec, dirn.)				$\nabla_h T$ (°C/100 km, dirn.)	
	Surface	200 m	500 m	1000 m	Calc. 200-500 m 500-1,000 m	Observed mea (M.O.483)
Trades (mean lat. 16°)	6·0, 127°	7·9, 124°	7·5, 121°	6·5, 118°	— — 0·4, 50°	0·4, (340°?)
Westerlies (mean lat. 48°)	2·9, 298°	5·6, 295°	6·3, 291°	7·1, 287°	0·9, 350° 0·6, 350°	0·8, 360°

7. Geostrophic departure and surface drag

The theory of ageostrophic motion due to friction may be briefly re-stated. Let u, v be the horizontal components of wind along rectangular axes x, y where x is directed along the surface wind u_0. If the pressure gradient, Coriolis force and internal friction are in equilibrium, the x-component equation of motion is

$$lv - \frac{1}{\rho}\frac{\partial p}{\partial x} + \frac{1}{\rho}\frac{\partial \tau_x}{\partial z} = 0$$

where l is the Coriolis parameter, ρ the density and τ_x the component of shear stress in the x-direction. Substituting the y-component v_J of the geostrophic wind for the pressure gradient term, the equation becomes

$$\frac{\partial \tau_x}{\partial z} = \rho\, l\,(v_J - v) \qquad . \quad . \quad . \quad . \quad . \quad (7.1)$$

and integrating

$$\tau_x(z) - \tau_x(0) = l\int_0^z \rho\,(v_J - v)\,dz \qquad . \quad . \quad . \quad (7.2)$$

where $\tau_x(0) = \tau_0$, the surface drag, since τ_0 is in the direction of the surface wind. The wind having been assumed to be without acceleration, Eq. (7.2) cannot be expected to have validity on individual occasions but may be expected to be valid in the mean of many cases, accelerations cancelling, provided the site is not meteorologically 'peculiar,' e.g., located in a standing eddy.

From Eq. (7.2), the surface drag may be evaluated from the integral of the geostrophic departure normal to the surface wind up to the level $z = h$ of zero τ_x. This

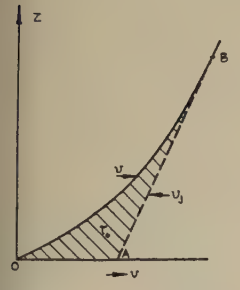

Figure 5. Determination of surface drag from geostrophic departure. Full line OB represents the vertical variation of the wind component v normal to the surface-wind direction and pecked line AB the corresponding component v_J of geostrophic wind, for v_J varying with height. If B is the level at which v becomes equal to v_J, area OBA is proportional to the surface drag provided $\tau_x = 0$ where $\partial\tau_x/\partial z = 0$.

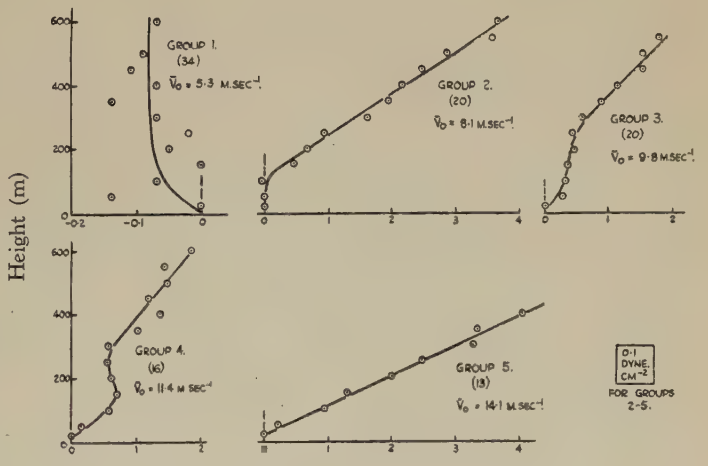
Height (m)
GROUP 1. (34) $\bar{V}_0$ = 5·3 M.SEC^{-1}
GROUP 2. (20) $\bar{V}_0$ = 8·1 M.SEC^{-1}
GROUP 3. (20) $\bar{V}_0$ = 9·8 M.SEC^{-1}
GROUP 4. (16) $\bar{V}_0$ = 11·4 M SEC^{-1}
GROUP 5. (13) $\bar{V}_0$ = 14·1 M.SEC^{-1}
0·1 DYNE CM^{-2} FOR GROUPS 2-5.
Wind component normal to surface wind (m sec^{-1})

Figure 6. Wind component v normal to surface-wind direction against height for the 5 groups of surface wind in Fig. 1. A component to the right of the surface wind is shown positive, i.e., the reverse of Eq. (7.2). Note the expanded abscissa for Group 1. Inset is shown an area corresponding to a drag of 0·1 dyne cm^{-2} for the scales of Groups 2-5 (cf. Fig. 2), but the geostrophic component v_J defining with v the area corresponding to total surface drag is not included.

level has usually been assumed to be the level for which $\partial\tau_x/\partial z = 0$ also and, therefore $v = v_J$ (see Fig. 5). The technique makes no assumption as to constancy of the geostrophic wind with height or of the mechanism of internal friction. It has been used by several workers, e.g., Taylor (1916), Sutcliffe (1936), Lettau (1950) to provide acceptable values of the drag of a land surface, but it has not yet provided values of the surface drag of the sea on which much confidence may be placed (see, however, Sheppard and Omar 1952).

Fig. 6 presents the variation of the component of wind normal to the surface wind at Scilly as a function of height for the five groups based on surface-wind speed (cf. Fig. 2). The curves are not of the form of the full-line curve of Fig. 5 and no corresponding component v_J of geostrophic wind can be inserted by inspection. The points of Group 1, based on no fewer than 34 ascents, would, if plotted to the same scale as for the other groups, effectively show zero values of v at all heights; with the abscissa expanded by a factor of 10 there is a small but probably significant component (about 0·1 m/sec) of sign opposite to that of the remaining classes and corresponding to the mean wind backing with height (cf. Section 5).

Before discussing the curves of Fig. 6 in relation to the values of v_J it is appropriate to enquire what minimum areas we might expect to be enclosed by the curves of v and v_J and the abscissa. For this some assessment of surface drag must be made from other data, however lacking in reliability they may be. There is a saving feature that the value of the drag arrived at must not be grossly inconsistent with the requirements of angular-momentum balance of the general circulation as recently studied numerically by Widger (1949), Priestley (1951), Starr and White (1951) and Mintz (1951). These studies weight the drag of higher winds, slightly if Neumann's $u_0^{3/2}$ law be assumed, considerably if Munk's or Durst's values be assumed, and imply a drag coefficient of something over 0·001 (values between 0·0013 and 0·003 have been used to obtain approximate balance between the meridional transfer of momentum and surface drag). If then we assume $\tau_0 = 0{\cdot}001\ \rho\ u_0^2$ for Groups 2-5 ($u_0 \geqslant 8$ m/sec) we are likely to obtain under-estimates of τ_0. The minimum values of τ_0 in the respective classes are then 0·8, 1·2, 1·6, and 2·5 dyne cm^{-2} respectively. The corresponding areas in Fig. 6 are indicated by a square equivalent to 0·1 dyne cm^{-2} so that if the shearing stress is to approach zero towards the top of the layer represented (in conformity with classical ideas,) the curves of v_J for the lower levels would need to be drawn well to the right of the curves for v on the scale to which the figure is drawn. It must be remembered however that the thermal wind, which is probably veered on the surface wind in Groups 2-5, would cause the v_J curves to slope upwards to the right, in the same sense as the curves for v. A very rough estimate suggests that the amount of slope might be as much as 1 m sec^{-1} (500 m)$^{-1}$.

Few workers have determined v_J directly, particularly over the sea. It is in fact very difficult to measure the horizontal pressure gradient, particularly in direction, with the precision which would be necessary to obtain τ_0 with some accuracy, assuming that Fig. 5, the classical picture, represents the actual behaviour of wind, i.e., that both τ_x and $\partial\tau_x/\partial z$ vanish at a few hundred metres height. Our estimates of the pressure field in the present series are certainly not that of precision but we are nevertheless able to infer with some confidence that *the actual values of* v_J *are quite insufficient to provide areas on Fig.* 6 *of the order indicated above.* The grounds for this statement follow.

We have made measurements of the surface geostrophic velocity by two methods

(i) from hourly values of pressure during the periods of soundings using three triangular arrays of pressure-reporting stations, (a) Valencia, St. Ann's Head, (Pembroke) and Scilly, (b) Scilly, St. Ann's Head, and Mount Batten (Plymouth), (c) Scilly, Mount Batten and Brest, the vector mean of the three triangles being used; (ii) from hourly synoptic charts during the periods of soundings. For the latter we are greatly indebted to Dr. A. G. Forsdyke of the Meteorological Office, Dunstable. The first method places too much reliance on the absence of significant error in the five station pressures, while the observing stations are not uniformly distributed in relation to the soundings – there is no station to the SW. of Scilly. In the second method it is difficult entirely to discount the wind when drawing up a pressure field to SW. of England though an assessment independent of wind is required. Dr. Forsdyke was aware of the requirement.

The results of the measurements and a comparison with the observed surface winds are given in Table III for each day of observation together with the mean of all days. (It was not practicable to obtain a mean pressure gradient for each group of soundings of Fig. 6 because groups are made up of soundings on different days).

TABLE III. COMPARISON OF SURFACE AND GEOSTROPHIC WIND VELOCITIES

Date	Geostrophic (J) Method (i)		Geostrophic (J) Method (ii)		Surface (S)		Direction diff. J-S		Speed ratio (S/J)	
Jan	m/sec	deg	m/sec	deg	m/sec	deg	(i) deg	(ii) deg	(i)	(ii)
3	10·0	212	9·0	205	8·1	223	− 11	− 18	·81	·90
4	13·7	233	10·0	227	7·8	226	7	1	·57	·78
6	18·0	269	17·7	281	14·0	266	3	15	·78	·79
8	6·7	267	6·7	271	9·8	268	− 1	3	1·46	1·46
10	5·8	284	5·9	287	5·2	297	− 13	− 10	·90	·88
11	13·8	263	15·4	262	9·3	255	8	7	·67	·60
12	19·0	265	14·4	269	10·8	273	− 8	− 4	·57	·75
						Means	− 2 ± 7	− 1 ± 8	·77 ± ·20	·88 ± ·16

Note: In the last four columns, (i) and (ii) refer to the method of obtaining the geostrophic wind. Mean deviations are given for the means at the bottom of these columns.

The differences in geostrophic speed and direction for the two methods of evaluation are as small as could be expected. The directions concern us particularly and while there is a difference of only 1° in the mean between the two sets of values this is attended by a mean deviation of ± 5° in the daily differences, so that undue weight should not be placed on the mean agreement. Turning to the comparison with the observed wind direction, the differences (J-S) vary from day-to-day quite appreciably and some of this variation is probably due to varying errors in the geostrophic-wind direction. The quite close agreement in the day-to-day change, upwards or downwards, in the two columns implies however that much of the variation is real and must then be ascribed either to the varying effect of friction or to variable ageostrophic effects due to other causes than friction. Assuming the latter to be more or less self-cancelling over the period the very notable result is left that the mean surface wind under friction is *veered* on the geostrophic by 1° or 2°, that is the wind is blowing *up* the gradient of pressure. Divorced from other evidence given earlier, this statement lacks reliability, but it is at least certain in the light of the mean deviations that there was no substantial mean down-gradient motion at the surface during the period of these flights.

We shall see below that this, at first, surprising conclusion of the absence o appreciable down-gradient motion at the surface is indeed consistent with othe evidence, including that given earlier. Moreover, it is confirmed as a climatologica feature, *in summer as well as winter*, for the westerlies of the North Atlantic, fro the independently derived fields of surface wind and pressure given in M.O.483 an there is some support for the result at Scilly in a climatological analysis by Sen (1925).

There is no inconsistency between this result and the mean values of speed rati (S/J) given in the last two columns of Table III which are near expected values, fo the wind speed must fall to zero (or ocean-current speed) at the surface. The signif cance of this ratio, which depends little in a direct sense on the angle of the wind wit the isobar, will be discussed in Section 10. We shall also refer there to the high valu of speed ratio on 8 Jan.

Table I above shows that the mean ratio of the 300 m to surface-wind speed i 1·21, and we then infer from Table III that the surface geostrophic wind speed i reached at or somewhat below the 300 m level. If the frictional layer be defined a the layer in which the mean wind speed is less than the geostrophic speed, the Scill observations give therefore about 300 m for this depth. But this is a more restricte definition of the frictional layer than is at present current in the literature.

Table III shows that it is quite impossible to draw lines of v_J in Fig. 6 whic with v will give areas of the order required to account for the surface drag. Indeec if the reality of the surface veer in the last row of Table III above be accepted, Eq. (7.1 implies that there is some *increase* of τ_x with height. Accepting some uncertainty i the mean angle of the surface with the geostrophic wind as given above it yet remain that *there is no substantial decrease of* τ_x *with height in the layer represented.* W therefore infer that the lower layers of the oceanic westerlies do not even approximat to the classical conception of the terrestrial barotropic boundary layer in which th shear stress diminishes with height to zero in the first few hundred metres, and remain zero at higher levels, the surface wind being measurably backed (northern hemisphere on the geostrophic. Some reason for the inadequacy of this structure has alread appeared. Additional reason is provided by the variation of turbulence with heigh as shown by our observations.

8. The variation of larger-scale turbulence with height

We have made a rough evaluation of the larger-scale turbulence and its variatio with height by treating the observations made on a given day (or part of a day, whe a marked and sustained change of velocity occurred during the day) as providing mean values of speed at each level, the individual flights providing the departures. W realize the shortcomings of such an analysis – arbitrary and variable period from which the mean is formed, non-objectivity in the definition of change of mean motion, finite and variable interval between soundings. But these shortcomings are by no means confined to the present series of observations; turbulence studies generally have some such limitations.

The turbulence evaluated by the technique adopted is roughly intermediate in scale between the time between soundings, 10 to 20 min, and the period over which the mean is formed, a few hours, but weighted rather strongly towards the former since substantial and sustained variations of velocity were eliminated by the choice of period. Fig. 7 shows the results for part or all of three days' flights and the mean for the seven days of observation.

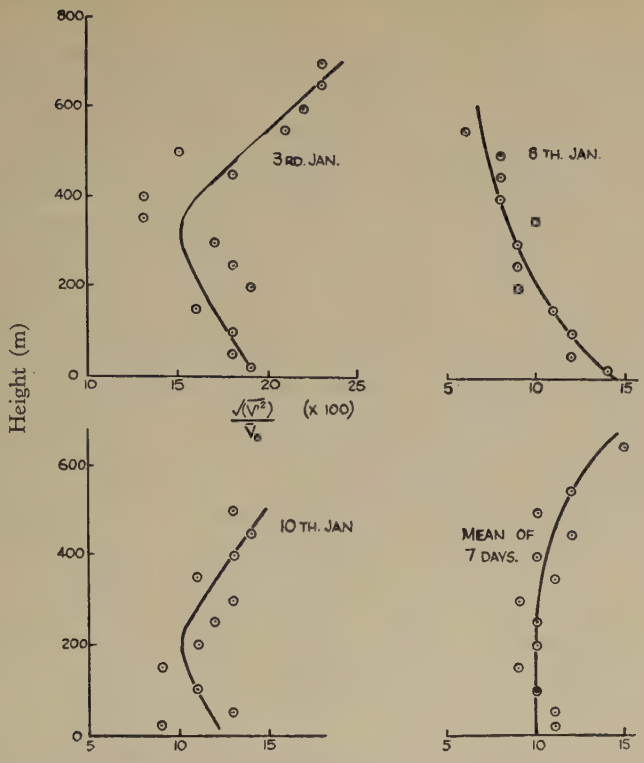

Figure 7. Curves of percentage turbulence against height for extended periods on three individual days and the mean of values obtained on all seven days.

The results vary a good deal from day to day, as Dines anemograms of surface wind for sea stations imply. The features which we regard as particularly notable are the generally high level of the percentage turbulence, and its maintenance and even increase in the mean over the whole layer examined. The latter feature is to be contrasted with the decrease of turbulence over the main part of a frictional boundary layer. There is of course no reason to suppose that turbulence of the scale examined here would follow the variation of frictional boundary-layer turbulence; eddies of large horizontal extent such as are picked out by the present analysis might be expected to have rather large vertical extent also, whatever their origin. This is probably convectional in part, but only in part if cloud form is to be the index. (Curiously, one day, 8 Jan., of appreciably convectional activity, with occasional showers, shows a systematic decrease of turbulence with height.) Clearly identifiable cloud form may not however be a sure guide to such turbulence. We envisage it as arising primarily from the larger-scale inhomogeneity in the horizontal field of temperature, that is as being primarily baroclinic in origin but not necessarily identifiable with any preferred form of atmospheric motion such as cumulus convection. The latter presumably demands close correlation between the temperature inhomogeneity at different levels which may often be absent.

Whatever be the source of energy of the larger-scale turbulence examined here, it is likely to be a major factor in exchange processes over substantial depths of atmosphere. It is probably more intense over the sea in winter because of the more active heat exchange between the sea and air in that season.

9. Momentum transfer in the westerlies

A qualitative explanation of the results obtained in the present study may now be advanced. Inasmuch as the turbulence discussed in the previous section is of convectional type, it will extend through substantial depths of the troposphere. It intensity will vary considerably with the synoptic situation and season but even it occasional presence will be evident in mean conditions. Moreover it is reasonable to suppose that momentum is transferred 'down the gradient' by such turbulent motion – there is some evidence for this in the reversals from the mean gradients of speed and direction presented in Section 6. There will then be a downward flux of westerly momentum at all levels in which the westerlies increase with height and in which turbulence is maintained.

Observations are not yet available to provide reliable values of the magnitude of the free-air transport of westerly momentum, $\rho\,\overline{u'w'}$ in the usual nomenclature, but it is easy to see that the stress produced by large-scale convective turbulence could well be of order 1 dyne cm^{-2} without calling for high correlation between u' and w' or insisting on a complete conservation of momentum. Or again, in terms of K theory, the observed mean gradient of the westerly component of wind at Camborne radar wind station during the Scilly work was 12 m/sec from 1 km to 5 km which, with $K = 10^5$ cm^2 sec^{-1}, – it could well be an order of magnitude greater – implies a stress of 0·3 dyne cm^{-2}. Thus, there is no difficulty in finding a free-air stress of the same order as the surface stress, and thus accounting for the observed relation between surface and geostrophic wind directions. This pattern of momentum transfer in the vertical is consistent with the correlation which was found between the speed ratio V_{300}/V_0 and the direction difference between the same levels, and with the fact that the horizontal temperature gradient could be roughly inferred from the vector variation of wind at quite low levels.

We have thus been led to a principle of momentum transfer of greater generality than that applying to the classical boundary layer : given shearing motion and turbulence there will be a transfer of momentum although the shear may arise from other causes than surface friction.

It is evident that the departure of the actual atmosphere from the barotropic frictional boundary layer will depend on the magnitude and direction of the thermal gradient relative to the surface-pressure field – particular associations will be discussed in a later paper, on the drag coefficient of the surface and on the intensity of larger-scale free-air turbulence. We know little about the latter at present. It is however possible to account for the barotropic frictional boundary layer being identifiable over land. The evidence depends partly on the absence of low cloud for the pilot-balloon observations and the atmosphere is then likely to be least baroclinic and least turbulent at free-air levels, while the drag coefficient of most land surfaces for the same reference level of wind speed is probably greater than for the sea. In combination these factors will make for greater shearing stress at low levels relative to that at higher levels, which is the main requirement. Note however that the effect of higher drag coefficient over land is partially offset by the surface wind for a given geostrophic wind being lower in that case.

A practical consequence of the result now reached is that it is not possible to arrive at reliable values of the surface drag from geostrophic departures without choosing appropriate upper-air conditions, and the geostrophic departure must be determined from the pressure field and not inferred from the upper wind. In a later paper it will

be shown that the low values of drag obtained previously for the ocean are due to the use of the upper wind for the geostrophic.

The above is somewhat qualitative but the physical picture of momentum transfer in the vertical by turbulence wherever there is a gradient of velocity, however engendered, is a reasonable extension of current ideas. No upper limit to the scale of turbulence effective in the transfer can be inferred from the present study and a complete picture must include the contribution from disturbances of synoptic scale for which the transfer may not be ' down the gradient.'

Without embarking on a major attack on the problem, the following considerations bring out some implications of the present thesis. Accelerations are again assumed absent, so that the equations refer to mean values over times sufficient to make the assumption valid, but there is no restriction on the variation of the pressure field with height.

The equations of motion for horizontal rectangular axes s, n along and normal to the flow respectively at *any* level, the wind speed being V and its angle with the geostrophic wind J being α (positive for J veered on V) are

$$0 = -\frac{1}{\rho}\frac{\partial p}{\partial s} + \frac{1}{\rho}\frac{\partial \tau_s}{\partial z}$$

$$lV = -\frac{1}{\rho}\frac{\partial p}{\partial n} + \frac{1}{\rho}\frac{\partial \tau_n}{\partial z}$$

or

$$\frac{\partial \tau_s}{\partial z} = -\rho l J \sin\alpha \quad . \quad . \quad . \quad . \quad . \quad (10.1)$$

$$V = J\cos\alpha + \frac{1}{\rho l}\frac{\partial \tau_n}{\partial z} \quad . \quad . \quad . \quad . \quad (10.2)$$

From Eq. (10.1) the *angle* which the wind makes with the isobar is determined by the vertical gradient of downwind shear stress, while from Eq. (10.2) the *difference of the wind speed from geostrophic* is, for α not too large, determined by the gradient of crosswind shear stress ($\cos\alpha = 1$ to within 3 per cent for $|\alpha| \leqslant 15°$). The sign of $\partial\tau_s/\partial z$ determines whether the motion is upgradient or downgradient but the magnitude of V compared with J is not directly influenced by upgradient or downgradient motion ($\cos\alpha$ always positive) and, when τ_n derives primarily from a thermal wind, there will be little indirect dependence.

From Eq. (10.1), with $J = 10$ m sec^{-1}, $l = 10^{-4}$ sec^{-1}, $\rho = 10^{-3}$g cm^{-3}, $\alpha = \pm 1°$

$$\frac{\partial \tau_s}{\partial z} = \mp 1{\cdot}7 \times 10^{-2} \text{ dyne cm}^{-2}\,(100 \text{ m})^{-1}$$

This variation is to be compared with a surface stress τ_0 for the given value of J, and c (in $\tau_0 = c\rho V_0^2$) $= 2 \times 10^{-3}$, of about 1·7 dyne cm^{-2}. Since the observed mean value of α was about 1° at the surface the relatively small change of stress in the first few hundred metres over the sea in the baroclinic westerlies is directly verified, with, of course, the possibility that such change as occurs may be positive rather than negative.

There are additional consequences to be inferred from Eq. (10.2). Thus V and $J\cos\alpha$ are always of the same order of magnitude but the crosswind shear-stress term may result in a significant difference between them. The shear gradient across the wind must in fact be exceedingly small in a turbulent atmosphere if the term is to

be negligible. For example, with a crosswind shear gradient of only 1 cm sec^{-1} (10 m)
and an eddy viscosity of 10^4 cm^2 sec^{-1}, or with 10 cm sec^{-1} (100 m)$^{-2}$ and 10^5 cm^2 se
respectively, the last term in Eq. (10.2) is about 1 m sec^{-1} and so comparable with th
other terms. Such crosswind shear gradients are however almost impossibly sma
to measure; certainly they cannot be extracted from Fig. 6. Again, a significant contr
bution to the shear-stress term may arise, particularly near the earth's surface, fro
the variation of eddy viscosity with height in association with crosswind shear. Fig.
indicates the latter to be of order 0·1 to 1 m sec^{-1} (100 m)$^{-1}$ and if the eddy viscosi
increases from 10^4 cm^2 sec^{-1} to 10^5 cm^2 sec^{-1} in the first 100 m, there is a contributio
to V of 1 to 10 m sec^{-1}. There is then no difficulty in accounting for the differen
between the observed ratio of V/J at the surface ($\simeq 0{\cdot}8$) and $\cos\alpha$ ($= 1{\cdot}0$ to with
less than 1 per cent) although the motion is substantially gradient or isobaric. Moreov
in Groups 2-5 of Fig. 6 the sign of crosswind shear is of the correct sense. It is al
perhaps more than a coincidence that the high value of V/J for the surface on 8 Ja
(cf. Table III) was associated with a reversal between 50 m and 150 m of the norm
crosswind shear, which could augment V by 2 m sec^{-1} for an increase of eddy viscosi
of 5×10^4 cm^2 sec^{-1} over that layer.

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on 'Atmospheric turbulence in the boundary layer,' Massachusetts Institute
Technology, June 1951, and to the International Association of Meteorolog
I.U.G.G., Brussels, August 1951.

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Appendix

Results of Balloon Observations over the sea near the Scilly Isles, 3-12 January 1951

α_0 = surface-wind direction, $T_a - T_0$ = difference between air and sea temperature. Wind speeds are given cm/½ min on plan trajectory (1 cm/½ min = 1·79 m/sec). Wind direction is given relative to α_0, veer positive. Valu in parenthesis of lower reliability – sometimes interpolated

Date and Time (GMT)	Run	α_0	$T_a - T_0$ (°F)	Wind speed (above) and direction (below) at heights (m) 20	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750	800
3 Jan																				
1120	2	222°	−0·5	3·5	3·7	3·7	4·3	3·4	3·6	3·7	4·4	5·0	4·2	4·7	4·6	4·1	5·1	4·5	4·6	4·6
				0	−9	−7	−7	−11	−11	−5	−9	−7	−5	−5	−5	−7	(−3)	(3)	(5)	(2)
1135	3	221°	−0·2	3·4	3·6	3·8	3·9	3·8	4·1	4·3	4·4	4·3	4·3	4·7	3·6	4·4	4·3	4·2	4·2	4·1
				0	0	−4	−15	−11	−9	−7	−7	−7	−7	−11	4	−1	−1	−1	0	−1
1150	4	228°	−0·7	4·2	4·9	5·1	5·0	4·9	4·8	4·9	5·1	5·1	4·5	4·6	4·1	4·2	4·4	4·8	5·3	5·0
				0	1	−1	−3	−3	−1	−4	−7	−5	−7	−3	−1	−1	3	7	5	6
1205	5	226°		4·6	4·8	4·3	4·7	5·0	4·7	5·2	5·1	5·3	4·8	5·4	5·2	4·6	5·5			
				0	−2	−7	−7	−6	−5	−1	−1	−3	−3	−8	−3	0	(5)			
1220	6	233°	−0·1	5·7	5·4	5·7	5·7	5·5	5·3	5·4	5·3	5·4	5·6	5·4	5·7	5·5	5·3			
				0	−1	−1	−2	−3	−6	−9	−1	−1	−3	0	+1	−1	(1)			
1300	7	217°	−0·3	5·4	5·4	5·4	5·7	5·9	6·0	6·0	6·0	5·9	5·9							
				0	0	1	3	7	7	7	8	11	9							
1315	8	217°	−0·3	4·1	5·3	5·4	(5·5)	(5·5)	5·3	5·6										
				0	−1	1	3	0	5	(5)										
1330	9	226°		4·9	5·8	5·8	6·3	6·3	6·1	6·0	6·1	5·9	5·9	5·9	5·7	6·1	6·1	5·6		
				0	0	−1	1	1	1	−1	3	−1	0	−1	−1	−3	−3	−1		
1345	10	225°	−0·2	5·6	6·5	5·6	6·2	6·5	6·7	6·7	6·5	6·7	6·7	6·7	7·0	7·1	7·6	7·5		
				0	1	−3	−1	−1	−1	−1	−1	−3	1	−1	−7	−2	−1	(3)		
1415	12	218°	−0·1	6·8	6·7	6·5	6·3	6·4	6·5	6·6										
				0	−1	0	4	3	3	(3)										
1430	13	220°		5·4	5·7	6·3	6·7	6·7	6·7	6·3	6·3									
				0	−3	−5	−1	0	−1	2	(3)									
1445	14	216°	−0·3	6·1	6·3	6·9	6·3	6·7	6·5	6·4	6·1	6·2	6·7	6·5	6·2	6·6	(7·1)	(7·1)		
				0	1	0	−1	−1	−1	−2	−5	1	−13	−5	(−6)	(−7)	−5	(−17)		
4 Jan																				
1145	17	226°	0·7	4·7	(6·9)	(6·9)	6·9	8·7	8·1	7·1	8·5	8·6	8·3	8·0	8·0	7·9	7·8	8·0		
				0	(−9)	(−18)	(−7)	−1	−5	19	(13)	(15)	(16)	18	19	19	20	21	(22)	
1250	21	237°	0·3	3·9	6·6	6·3	6·5	6·5	6·7	6·8	7·1	7·2	7·1	6·9	8·0	7·9				
				0	−13	−3	5	3	5	4	3	5	3	1	3	−1	(9)			
1310	22	238°	0·9	3·9	6·7	6·1	7·2	7·3	7·5	7·7	7·8	7·8	8·3	7·8	7·9	8·0	7·8	7·6		
				0	−3	−5	−6	−3	(3)	1	0	2	1	1	3	5	4	3		
1335	24	221°	1·4	4·4	5·4	5·9	6·9	7·0	(8·0)	(7·6)	7·8	7·6	7·3	7·4	7·6	8·1	8·1	6·9		
				0	−2	0	7	9	9	14	15	15	15	17	19	(23)	(23)	25		
1405	26	223°	1·1	4·6	5·5	6·3	6·4	7·2	7·3	7·4	7·5	7·3	7·1	7·2	6·9					
				0	−3	−3	−2	1	3	7	7	9	11	17	(17)					
1420	27	216°	0·8	4·3	5·1	6·6	6·5	6·8	7·2	6·9	6·6	7·2	7·4	7·8	8·5	8·5	8·2			
				0	3	1	7	8	11	17	21	27	31	33	35	35	35			
1435	28	222°		4·6	5·1	5·7	6·3	6·6	7·1	7·7	6·2	6·7	7·0	7·0	7·1	7·2	7·0	6·5	7·0	7·0
				0	0	1	1	−1	5	7	15	15	18	21	23	27	31	39	36	35
6 Jan.																				
1315	37	244°	2·2	7·8	7·2	(8·5)	(8·5)	(8·7)	(8·7)	9·0	9·3	9·2	9·6	9·8						
				0	3	(7)	(15)	(15)	(25)	27	28	27	27	27						
1325	38	249°		7·7	7·0	8·4	(8·5)	(8·5)	(9·1)	(9·1)	9·1	9·4								
				0	3	5	(9)	(13)	(13)	(19)	21	21	(23)							
1335	39	247		(7·6)	(7·6)	7·4	(8·6)	(9·0)	(9·0)	(9·0)	(9·0)	(9·0)	8·6	8·8	(8·7)	(8·6)				
				0	0	9	13	(19)	(19)	(19)	(19)	(19)	(22)	25	25	(25)				
1345	40	245°	1·3	(8·1)	(8·1)	7·7	8·3	(9·9)	(9·9)	9·4										
				0	0	13	13	(17)	(17)	24										
1410	41	251°	1·2	(7·4)	(7·4)	7·8	8·4	9·4	9·0	8·8										
				0	(0)	6	5	13	5	(7)										
1445	43	272°	0·5	(7·7)	(7·7)	8·0	8·4	8·4	8·3											
				0	1	3	3	(4)												
1455	44	279°	−0·6	7·5	7·9	7·9	(7·9)	(7·9)												
				0	3	5	5	(7)												

te d me (GT)	Run	α_0	$T_a - T_0$ (°F)	Wind speed (above) and direction (below) at heights (m) 20	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750	800
n.																				
1515	**46**	289°		(7·7)	(7·7)	7·7	(8·3)	(8·8)												
				0	0	−5	−2	1												
1540	**48**	299°	−2·5	8·9	10·0	9·8	9·7	9·2	9·5											
				0	5	5	5	5	5											
n.																				
1030	**50**	240°		4·1	4·7	4·2	4·6	4·9	4·7	4·9	(5·3)	(5·8)								
				0	3	−2	−1	1	2	3	5									
1040	**51**	235°	−0·2	4·5	5·0	5·0	5·2	5·4	5·6	5·8	5·9	6·1	6·3	6·3	6·4					
				0	5	6	7	9	13	15	17	19	24	(23)	(23)					
1050	**52**	237°		4·3	4·4	4·5	4·9	5·3	5·5	5·6	6·0	6·3	6·5	6·2	6·0	5·7	5·7	5·7	6·0	
				0	1	1	3	7	9	13	19	21	21	23	25	25	26	31	(33)	
1110	**53**	244°	0·1	4·9	5·0	5·1	5·7	5·9	6·1	6·0	6·7	6·6	6·5	6·3	6·0	4·5				
				0	1	6	9	7	9	15	17	13	19	23	29					
1120	**54**	243°		5·1	5·4	5·7	5·9	6·3	6·3	6·3	6·5	6·7	6·7	6·7	6·1	6·3				
				0	2	8	11	11	15	18	25	27	31	31	33	34				
1130	**55**	245°	1·8	5·2	5·0	5·3	5·5	6·3	6·3	5·7	6·3									
				0	7	7	5	11	11	13	15	18								
1140	**56**	265°	0·1	6·4	6·1	(6·4)	7·1	6·7	6·5	6·9										
				0	5	7	2	3	3	2										
1150	**57**	277°		5·4	5·5	6·0	6·4	6·4	6·7	6·3	6·5	6·5	6·7	6·2	6·1	6·1	5·7			
				0	0	−3	−1	1	2	5	7	11	11	3	5	3	(0)			
1200	**58**	281°	0·7	5·0	4·9	4·8	5·1	5·1	5·3	5·5	5·5	5·2								
				0	0	−1	−1	−3	−1	1	1	−3								
1210	**59**	283°		5·2	5·9	5·7	5·9	5·8	5·7	5·7	5·7	5·8	5·8	5·8						
				0	3	3	1	3	3	3	5	5	7	5						
1220	**60**	288°	0·9	4·9	5·9	6·0	5·7	5·5	5·5	5·3	5·1	5·2	5·4	5·3						
				0	2	1	−1	−1	−1	−5	−5	−3	−6	−5						
1230	**61**	283°		4·5	5·1	5·2	5·9	5·7	5·7	5·7	5·7	5·9	5·8	5·5						
				0	3	3	3	2	2	3	1	1	(−3)	(3)						
1240	**62**	283°	0·3	5·1	5·1	5·1	5·5	5·3	5·4	5·4	5·5	5·7	5·7	5·5	5·7					
				0	−5	−5	−3	−3	−5	−5	−5	−3	−5	−5	0					
1250	**63**	283°		5·8	5·7	5·9	5·8	5·7	5·5	5·6	5·7	5·8	5·9	6·1	6·1					
				0	3	1	−1	−3	−5	−9	−9	−12	−9	−7	−3					
1310	**64**	280°	0·4	6·6	6·1	6·4	6·1	6·4	6·1	5·8										
				0	−5	−7	−7	−6	−7	−5										
1320	**65**	274°		6·2	6·7	6·8	6·3	6·6	6·5	6·3	6·4	6·6	6·1							
				0	−3	−1	−1	−1	−1	1	3	0	5							
1330	**66**	270°	0·6	5·9	5·8	6·3	6·3	6·8	6·5	6·3	6·7	6·4	6·4	6·5	6·6	6·8	7·1			
				0	5	1	3	7	3	5	5	5	7	5	9	5	7			
1340	**67**	270°		6·0	5·7	5·8	5·9	5·8	5·9	5·9	6·3	6·3	6·5							
				0	7	3	3	5	−2	−1	−1	3	3							
1350	**68**	268°	0·0	5·7	5·7	6·3	6·3	6·6	6·5	6·5	6·9	7·1	6·9							
				0	5	6	5	5	7	6	9	9	10							
1400	**69**	266°		5·8	6·3	6·1	6·3	6·3	6·4	6·3	6·4	5·8	5·7	5·8	6·5					
				0	3	4	3	3	4	4	1	5	5	7	9					
1410	**70**	265°		5·0	4·8	5·3	5·6	5·5	5·6	5·7	5·5	5·7	6·2	6·5	6·5	6·7				
				0	3	0	−3	−3	−3	−4	−5	−4	−1	1	1	3				
1420	**71**	261°	−1·1	5·7	5·6	5·8	6·1	5·9	5·5	5·5	5·7	6·0	6·4	6·3	6·3	7·0				
				0	3	5	3	3	3	7	2	3	5	3	5	7				
1430	**72**	264°		6·4	5·7	5·9	6·1	5·9	5·8	6·2	6·0	5·8								
				0	1	−1	−3	−2	−5	−5	−4	−2								
1440	**73**	264°	−0·7	6·3	5·6	5·5	5·4	5·6	5·5	5·6	5·7	5·7	5·9	6·2	6·3	6·7	6·4			
				0	9	11	11	5	1	1	5	5	4	5	3	0	1			
1450	**74**	266°		7·3	7·1	6·8	7·0	6·7	6·6	6·7	7·0	6·9	7·5	7·4	7·3					
				0	−2	−1	1	0	2	5	3	6	4	(5)	(5)					
1505	**75**	277°	−0·1	6·0	6·5	6·6	6·7	6·3	6·6	(6·7)										
				0	−5	−8	−9	−7	−8	(−11)										
1515	**76**	291°		9·0	9·1	9·2	8·4	8·6	8·9	8·7	8·9									
				0	0	−1	−2	−4	−6	−5	(0)									
1525	**77**	287°		8·5	8·5	8·9	8·7	8·7	8·9	8·6	8·5	8·6	8·6							
				0	−6	−4	−7	−3	−1	3	4	5	3							

te d me (GMT)	Run	α_0	$T_a - T_0$ (°F)	Wind speed (above) and direction (below) at heights (m) 20	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750	800
Jan.																				
1030	**78**	326°	−4·0	3·4	3·4	3·4	3·3	3·4	3·3	3·3	3·3	3·6	3·7	3·5						
				0	1	7	5	5	8	5	1	4	4	8						
1040	**79**	330°	−3·9	3·2	3·5	3·8	3·1	3·5	3·5	3·9	3·8	3·9	3·9	3·8	3·5	3·9	3·8	3·5	3·5	
				0	−3	−5	−5	−1	−2	−1	−1	3	7	9	4	11	10	7	10	
1050	**80**	329°	−3·9	3·0	4·0	3·7	3·8	3·6	3·8	3·7	3·5	3·7	3·3	3·3	3·3	2·9				
				0	0	−1	5	5	3	5	2	1	0	4	7	3				
1105	**81**	336°	−2·9	3·3	3·4	3·2	3·7	3·4	3·6	3·7	3·6	4·2	4·0	4·1	3·7	3·6	3·5	3·4	3·3	3·5	3·3
				0	−7	−3	7	−3	−7	−11	−7	−9	−11	−13	−13	−15	−17	−20	−24	−9	−13
1120	**82**	317°	−3·6	3·4	3·5	4·1	3·5	3·9	3·7	3·6	3·3	3·5	3·5	3·7	3·9	3·8					
				0	1	3	3	3	0	2	−1	3	8	10	14	11					
1130	**83**	319°	−3·3	2·8	3·0	3·1	3·2	2·8	2·9	3·5	3·5	3·6	3·1	3·7	3·3	3·8	3·4				
				0	−2	−3	1	−11	−6	−1	−9	6	−4	8	5	10	9				
1140	**84**	316°	−2·5	3·3	3·1	3·0	3·7	3·9	3·6	3·5	3·3	3·1	3·3	3·4	3·2						
				0	7	9	5	7	7	4	3	9	2	4	0						
1150	**85**	314°	−2·8	3·7	3·8	3·7	3·6	3·7	3·7	3·7	3·8	4·1	4·2	3·8	3·9	4·0	3·9	3·9			
				0	3	3	3	2	−1	0	3	5	5	4	4	4	5	4	3		
1200	**86**	316°	−2·2	3·4	3·6	3·3	3·3	3·5	3·5	3·3	3·4	3·2	3·3								
				0	−1	−2	−1	1	4	0	8	3	2								
1215	**87**	317°	−2·2	3·1	2·9	3·1	2·9	3·2	2·9	2·9	3·1	3·1	3·3	3·0							
				0	0	1	1	−5	−5	−6	−15	−5	−6	−7							
1225	**88**	320°	−2·0	3·3	3·7	3·4	3·4	3·3	3·3	3·4	3·3	3·2	2·9	2·7							
				0	−6	−2	−4	−5	−3	−5	−7	−11	−6	−1							
1235	**89**	315°	−2·8	2·6	2·5	2·9	3·5	3·1	3·3	2·9	3·0	3·1	2·9	3·0	3·0	2·8	2·9	2·8			
				0	−3	−6	−9	−11	−7	−3	−4	3	5	10	1	5	6	12			
1245	**90**	330°	−1·9	2·9	2·7	2·8	2·8	2·5	2·3	2·2	2·3	2·5	2·4	2·6	2·5	2·9					
				0	−7	−8	−15	−13	−15	−20	−20	−18	−18	−20	−20	−20					
1305	**91**	309°	−1·8	2·7	2·6	2·4	2·5	2·7	2·7	2·6	1·9	2·2	2·3	2·3	2·5						
				0	−4	0	0	−1	3	2	14	6	11	4	9						
1320	**92**	299°	−1·6	2·7	2·7	3·0	2·9	2·6	2·6	2·4	2·5	2·3	(2·4)								
				0	0	−4	5	4	1	−2	−2	7	1								
1330	**93**	311°	−1·8	2·4	2·5	3·1	2·7	2·7	3·1	3·1	3·2	2·8	2·7	2·6	2·6						
				0	2	3	10	0	3	5	3	−6	−8	−3	−7	−5					
1340	**94**	288°	−1·9	2·1	2·7	2·8	2·9	3·4	2·9	2·9	2·9	2·9	2·9								
				0	4	8	13	23	12	19	25	22	24								
1350	**95**	301°	−2·1	2·6	2·7	3·1	3·1	3·5	2·9	2·9	3·1	2·6	2·7								
				0	2	−3	−1	−1	−3	−3	−7	0	3								
1400	**96**	294°	−2·1	3·0	2·5	2·4	2·3	2·4	2·6	2·8	2·6	2·9	2·7	2·7	2·4						
				0	1	5	9	1	6	12	3	3	5	5	3						
1415	**97**	294°	−1·7	2·8	2·7	2·7	2·6	2·5	2·7	2·6	2·6	2·6	2·7	2·6							
				0	2	2	1	−2	−8	−16	−16	−25	−17	−15							
1425	**98**	297°	−1·9	2·5	2·7	2·9	2·9	3·0	2·6	2·8	2·6	2·5	2·6	2·6							
				0	1	1	1	−7	−7	−12	−20	−15	−15	−13							
1435	**99**	289°	−1·7	1·7	1·6	1·8	1·9	2·3	2·2	2·2	2·2	2·4	2·1	2·4	2·3	2·6	2·5	2·3			
				0	0	1	−3	0	0	1	−11	−7	−7	2	3	−6	−7	−15			
1445	**100**	267°	−1·9	2·4	2·3	2·5	2·5	2·1	2·1	2·1	2·5	2·6	2·4	2·2	2·3						
				0	0	3	6	9	14	11	9	3	−2	1	5						
1505	**101**	261°	−2·7	2·5	2·7	2·5	2·5	2·4	2·5	2·6	2·5	2·5	2·7	2·6	2·6						
				0	3	−3	−1	−1	1	−5	−1	3	−1	−1	0						
1520	**102**	256°	−3·2	3·0	2·9	2·8	2·8	2·9	2·8	2·7	2·7	2·6	2·5								
				0	1	3	3	3	3	4	−1	0	−11								
1530	**103**	248°	−3·4	2·4	2·4	2·7	2·7	2·7	2·5	2·5	2·7	2·5	2·8								
				0	−4	−7	−5	−7	−2	−11	−5	−12									
1540	**104**	238°	−3·7	2·7	2·9	2·9	3·2	3·0	3·1	2·9	3·2	3·2									
				0	0	1	3	5	−1	−1	−5	−3									
1550	**105**	227°	−4·0	3·1	3·2	3·3	3·1	3·0	3·1	3·1	3·3	3·2	3·1								
				0	5	2	7	11	7	9	10	11	(7)								
1600	**106**	236°	−3·7	3·4	3·1	3·3	3·5	3·6	3·5	3·4	3·5	3·6	3·6	3·7	3·7	3·8					
				0	−7	−3	−9	−8	−7	−9	−11	−10	−11	−12	−11	−17					
1615	**107**	229°	−3·7	3·1	3·4	3·6	3·6	3·8	3·9	3·9	3·8	4·0	3·7	3·9	3·8	3·9	4·3				
				0	3	1	1	1	3	1	4	−2	−1	−1	−2	−3	4				

Run	α_e	$T_a - T_e$ (°F)	Wind speed (above) and direction (below) at heights (m) 20	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750	800
5 111	269°	−4·0	(5·5)	(5·5)	6·0	5·4	5·0	5·1	5·4	5·7	5·8	6·1	6·0	6·4	6·2	6·8	6·8	7·3	7·0
			0	(0)	−2	−7	−10	−11	−11	−9	−7	0	3	3	5	6	7	(9)	(9)
0 112	252°	−3·9	4·4	5·3	5·1	5·4	5·6	5·9	5·9	5·9	5·9	6·0	6·7	6·5	7·0				
			0	−1	−5	−5	0	−1	4	5	4	5	7	9	16				
0 114	253°	−4·6	5·9	6·0	6·3	6·7	7·1	7·2	7·4	6·9	7·5	6·9	7·5	7·6	7·3	8·1	8·9	6·3	7·5
			0	3	5	4	3	5	7	15	17	22	25	27	25	27	25	28	29
5 115	243°	−4·0	(5·5)	(5·5)	6·2	6·7	6·9	7·1	7·1	7·5	7·1	7·8	7·5	7·8	7·0	6·4			
			0	(5)	11	15	17	18	25	27	28	29	30	30	35	36			
0 116	261°		(4·8)	(5·2)	(6·0)	(6·6)	6·9	6·7	7·1	7·2	6·9	6·7	7·5	7·2	7·0	7·1	7·1	6·9	6·6	7·1	7·9
			0	(0)	(0)	2	5	(7)	3	3	7	9	16	17	17	17	18	19	21	22	23
0 117	264°	−4·2	4·7	6·7	8·6	9·5	9·8	9·6	10·1	(9·6)	(9·7)	9·4	10·5	8·9					
			0	0	5	9	7	8	10	(17)	(17)	19	18	15					
0 118	287°	−3·8	6·2	8·5	8·5	8·3	8·6	9·0	9·1	8·9	8·7	8·8	9·1	9·3					
			0	2	5	3	3	3	1	−2	−3	−5	−5	(−5)					
5 119	271°	−3·7	4·9	6·7	(9·1)	(9·6)	(9·6)	(9·7)	9·8	10·0	10·0	9·3	10·2	10·3	11·6	10·6	10·9	11·4	
			0	2	(3)	(2)	(2)	(2)	7	7	7	8	11	13	14	17	16	17	
5 120	269°	−2·7	7·5	8·3	8·9	(9·8)	(9·9)	10·4	10·7	10·8	10·3								
			0	3	7	(5)	(5)	9	7	8	9								
1 124	273°	−3·7	6·5	8·0	9·4	9·8	9·8	10·1	10·0	10·7	10·7	10·6	10·6	10·3	9·1				
			0	1	12	13	13	12	14	14	17	18	16	17	16				
5 125	277°		6·4	9·5	9·7	8·8	9·1	9·9	10·2	9·7	10·3	10·1							
			0	−1	3	8	9	11	9	9	15	9							
5 126	277°	−2·2	(6·8)	(7·6)	(8·0)	8·5	8·9	8·9	9·3	10·5	10·8	8·4	9·9	11·7	9·8				
			0	(0)	(3)	6	5	3	1	3	2	−1	4	5	5				
5 127	274°	−1·2	(6·5)	(7·5)	9·7	10·1	10·5	10·2	10·5	10·3	10·5	11·3	10·4	10·4	11·4	10·9			
			0	1	3	4	−1	1	3	6	3	6	7	9	8	(6)			
0 129	269°	−2·9	(6·4)	(7·5)	(9·0)	9·4	10·0	10·7	9·9	10·0	10·1	10·0	10·3	10·6	10·2				
			0	(0)	(1)	3	1	5	5	7	8	8	9	9	7	(7)			
0 131	267°	−1·6	(4·6)	(6·6)	7·9	(9·1)	(9·1)	11·5	9·9	9·9									
			0	0	5	(11)	(11)	(11)	9	9									
0 132	271°	−0·7	(5·4)	(7·1)	8·9	9·1	9·9	9·9	11·2										
			0	0	5	4	3	2	3										
3 134	276°	−2·0	(6·5)	7·7	8·7	9·3	9·3	9·5	10·1										
			0	0	3	5	5	3	5										

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The wind stress over the ocean from observations in the Trades

By P. A. SHEPPARD and M. H. OMAR
Imperial College, London

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SUMMARY

The method of geostrophic departure is used to determine the surface drag of the ocean in the Trade-wind belt, this region being particularly suitable for the technique because the opposition of the thermal wind to the surface geostrophic wind leads to a maximum in wind speed at a few hundred metres elevation. This pilot study is based on balloon winds at three very small Pacific islands and the U.S.W.B. Northern Hemisphere Charts of surface pressure. A drag coefficient of $1{\cdot}0 \times 10^{-3}$ is found from two of the stations, and $1{\cdot}5 \times 10^{-3}$ at the third and there is no systematic variation with surface-wind speed from 3 m/sec to 12 m/sec. The higher value of drag coefficient may be due to excessive roughness caused by reefs, or possibly to thermal instability.

A similar analysis for San Juan in the West Indies shows the disturbing effect of a larger land area on the geostrophic departure so that only in high surface winds does the apparent drag tend to the probable value. Some other features of the Trade-wind regime are briefly described.

1. INTRODUCTION ; TECHNIQUE

The wind stress over the oceans is an important factor in numerical studies of the mechanics of the general circulation but its magnitude and dependence on surface-wind speed are still quite uncertain (Charnock 1951). Recent work (Sheppard, Charnock and Francis 1952) has indicated that the variation of wind with height due to meridional temperature gradient makes it almost impossible to evaluate the surface drag from geostrophic departure of wind in the westerlies, but the same circumstance provides specially favourable conditions for its determination by that technique in the Trades. The present paper gives a preliminary evaluation of drag from routine observations of wind and pressure in low latitudes, to demonstrate the possibilities of the technique. More refined observations are required to exploit the technique fully and to provide confirmation and extension of the results which are presented below.

In the absence of acceleration, the surface drag τ_0 is related to the wind by

$$\tau_0 = -l \int_0^h \rho\,(v_J - v)\,dz \qquad . \qquad . \qquad . \qquad (1)$$

where l is the Coriolis parameter ($2\,\omega \sin\phi$), ρ the density, v_J and v the components of the geostrophic and actual wind normal to the surface wind direction, z the height above the surface and h the height at which the shear stress in the direction of the surface wind vanishes. The latter is given by the level at which $\partial u/\partial z = 0$ where u is the component of wind in the direction of the surface wind.

Wind as a result of balance between geostrophic and frictional control, the basis of Eq. (1), is considered to be realized in the mean flow in the latitudes of the Trades, for, whereas the Coriolis parameter is low, the winds are relatively steady. Moreover inspection of mean flow charts shows that the path acceleration is probably an order of magnitude below the Coriolis acceleration.

There is in general no level of $\partial u/\partial z = 0$ in the westerlies below the near-tropopause maximum of u but, on account of the opposition between surface and thermal wind in

the Trades, the level h there usually occurs below 1 km. With such a value it becomes possible to measure the integral of $(v_J - v)$ with reasonable accuracy, provided the surface-pressure field may be determined satisfactorily. It is important to note that $v_J \neq v$ at $z = h$ unless the shear stress in the direction of the surface wind remains zero above h ; this is not in general even approximately true (Riehl *et al.* 1951, Sheppard unpublished) and the assumption of $v_J = v$ at $z = h$ has led earlier workers to quite false values of τ_0 in consequence.

There are good series of observations of wind in the lower atmosphere of the Trades, e.g., from the 'Meteor' expedition, but unfortunately the accompanying pressure fields are not available. We have therefore gone to the post-World-War II *Daily Series Synoptic Weather Maps* (U.S.W.B.) to obtain the surface-pressure field and surface and upper winds for certain small Pacific islands and one Atlantic island lying in the Trades. Observations have been extracted over the period Jan. 1949 to Apr. 1950 and have been used in Eq. (1) to provide τ_0.

2. The analysis

(*a*) *Selection of stations*

We required winds which were representative of those over the ocean and accordingly sought observing stations on very small, low-lying islands remote from land masses, in regions moreover in which a reasonable network of pressure-observing stations would give some reality to the isobars on the charts. Three such islands were found in the Pacific, relevant particulars of which are given in Table I.

TABLE I. Particulars of Pacific Islands used in study

Island	Lat.	Long.	Area (km^2)	Height (m)	Remarks
Wake	19°18′N	166°37′E	10	0-6	Mainly tree-covered. Immediately surrounded by coral reef.
Johnson	16°44′N	169°31′W	0·4	0-12	Atoll. Surrounded by coral reefs to about 10 km distance.
Eniwetok	11°21′N	162°21′E	2	0-4	Tree-covered atoll. Coral reefs to N. and NW.

No suitable observing station was found in the Atlantic but it was thought likely to be of interest to see in what respects a much larger island would influence the results. San Juan (18°28′N, 66°07′W, 25m above M.S.L.) on the island of Puerto Rico was available. Puerto Rico is roughly 100 mi E-W by 40 mi N-S, and San Juan which lies on the NE. coast is thus open to the NE. Trades. Unfortunately for our present purpose the island is mountainous, rising behind the northern coastal plains to a maximum height of over 4,000 ft.

(*b*) *Selection of soundings*

Winds were reported to the nearest knot in speed and 10° in direction at levels 0, 1, 2, 4, 6 . . . thousand feet. Our aim being to obtain reliable values of the wind

stress at given wind speeds, and not climatological values, soundings were selected to this end on the following basis :

(i) Surface wind direction between 0° and 150° (San Juan 0° to 90°). (ii) A maximum in u at a level at or below 4,000 ft (2,000 ft for Wake Island, for which soundings were more numerous). When two maxima were present below 6,000 ft, only those soundings were selected in which the lower maximum was the major, and the upper maximum was not more than 3 kt greater than the preceding minimum.

(c) *Computations from soundings*

The soundings from each station were classified into four groups according to the surface-wind speed, such that an approximately equal number of soundings (*c.* 40*) was available in each group. As the distribution of surface-wind speed was different for each station, the wind-speed limits for the groups differ station to station.

The wind components u and v as defined in Section 1 were evaluated for each reported level and mean values were formed for each level and group of soundings. Mean values of h for the groups were then read off the smoothed curves, as indicated in Fig. 1. There is necessarily some uncertainty in the value of h but the error is hardly likely to exceed about 15 per cent in most cases (error greatest in low wind-speed groups) and is unlikely to be systematic.

(d) *Determination of geostrophic wind*

Two quantities are required, the value of v_J at the surface and the increment of v_J over the height h. Since h is a few hundred metres only, the latter may be inferred

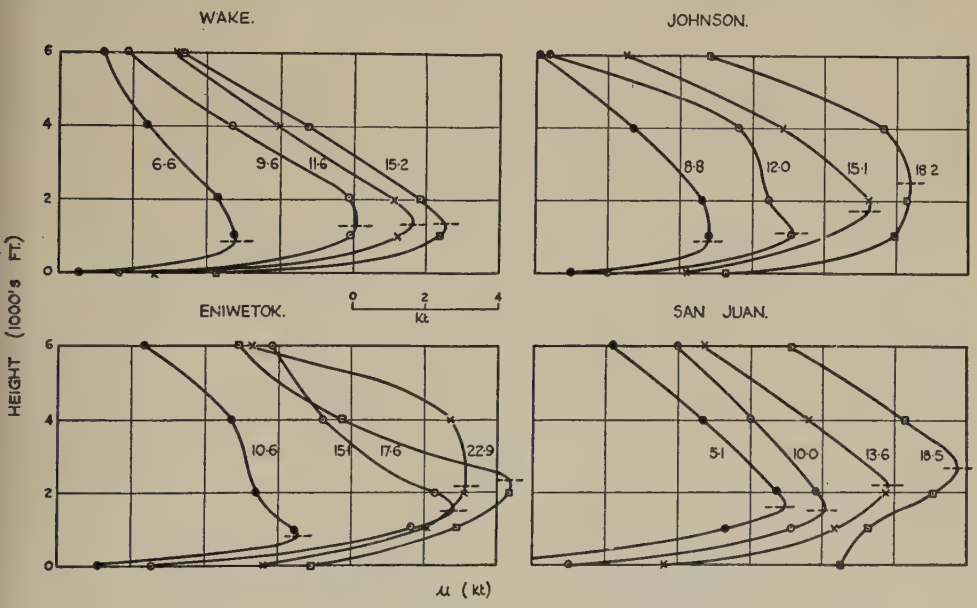

Figure 1. Profiles of the component u of velocity in the direction of the surface wind for soundings grouped according to the surface-wind speed. The mean value of the latter in knots for each group is shown against the curve, on which is also shown by broken line the assumed level of maximum u.

* Except for Eniwetok for which there were no soundings from Jan.-June 1949.

from the field of surface temperature using the approximate form of the thermal wind equation,

$$\Delta v_J = \frac{g}{lT} \frac{\partial T}{\partial x} h$$

where the x-axis is along the surface wind.

Isobars are drawn at 5 mb intervals on the Northern Hemisphere Charts and the weak pressure gradients of low latitudes then result in a wide separation of the isobars sometimes up to 10° in latitude. This, and the sparsity of observations of pressure on which the isobars are based make this part of the analysis quite the least reliable and it can only be hoped that sufficient occasions were taken to reduce the errors of the means to reasonable limits. (Eniwetok, the lowest latitude station, is probably the least reliable). Internal consistency is here the only gauge.

The error in evaluation of the temperature field was also considerable but this is of little consequence since it provides only a small contribution to the integral to be evaluated.

The charts were prepared for the epoch 1230 GMT whereas the balloon soundings referred to 03 GMT. In consequence the surface values of v_J and of $\partial T/\partial x$ were obtained as means from the two charts on either ' side ' of the balloon-sounding hour.

(e) *Evaluation of surface drag and drag coefficient*

The surface drag τ_0 was determined for each group of soundings by graphical evaluation of the integral in Eq. (1) using a constant value of air density $\rho = 1{\cdot}2 \times 10^{-3}$ g cm^{-3}.

The corresponding drag coefficient c for each group is given by

$$c = \frac{\tau_0}{\rho_0 \overline{u_0^2}}$$

where $\overline{u_0^2}$ is the mean square of the surface wind speed in the selected range of u_0 and ρ_0 has also been taken to be $1{\cdot}2 \times 10^{-3}$ g cm^{-3}.

3. Results and discussion

The results of the measurements and computation are given in Table II and we consider first those for the three Pacific Islands. Here the drag coefficients at each station are sensibly independent of surface wind speed – they are in fact surprisingly constant in view of the uncertainty in the individual values of v_J – but the Johnson Island values are 50 per cent greater than for the other two islands. The low values of c for the low wind-speed range at Wake appears to be due to the relatively high number of individual cases in which the surface wind blew towards high pressure, as shown on the charts, and the low value of c for the high wind-speed range on Eniwetok is possibly due to the smallness of sample.

It will be noted that the contribution of v to the integral in Eq. (1) is always quite small so that the surface drag is given very nearly by the surface-wind direction and pressure field, and the height h. This is directly opposed to assumptions made by earlier workers in the field. It will also be noted that the wind backed with height at Eniwetok in the lower speed ranges and by much more than the thermal wind, as evaluated. This variation is unexplained.

TABLE II. WIND, SURFACE DRAG, AND DRAG COEFFICIENTS

Station	Range of u_0 (knots*)	Number of ascents	$\bar{u}_0$ (m/sec)	$\overline{u_0^2}$ (m/sec)^2	h (m)	$-vj$ at $z=0$ (m/sec)	Δvj (m/sec)	$-v$ at $z=h$ (m/sec)	τ_0 (dyne/cm^2)	$c \times 10^3$	Annual mean $T_a - T_0$ (°F)
1. Wake Island	3-8	40	3·4	11·9	260	1·6	·0	·2	·10	0·8	0·3
19°18′N, 166°37′E	9-10	50	4·9	24·5	380	3·6	·1	·7	·31	1·0	
	11-12	34	6·0	35·7	410	2·0	·1	·5	·44	1·0	
	13-19	35	7·8	62·3	410	3·6	·1	·5	·74	1·0	
2. Johnson Island	4-10	42	4·5	21·2	240	3·3 ± 3·3†	·1	·2	·39	1·5	−0·3
16°44′N, 169°31′W	11-13	32	6·2	38·3	300	4·7 ± 2·1	·2	·6	·66	1·4	
	14-16	35	7·8	60·7	500	5·6 ± 3·2	·3	1·1	1·10	1·6	
	17-21	43	9·4	88·1	760	5·3 ± 3·7	·5	2·0	1·57	1·5	
3. Eniwetok Atoll	5-12	20	5·4	30·7	240	3·6	·1	−2·1	·41	1·1	0·1
11°21′N, 162°21′E	13-16	19	7·8	60·8	440	4·6	·4	− ·7	·73	1·0	
	17-19	21	9·1	81·8	610	4·9	·1	·2	1·10	1·1	
	21-26	15	11·8	139·3	700	5·6	·6	2·1	1·01	0·6	
4. San Juan	2-7	24	2·6	7·6	490	3·5	−·1	·8	·85	9·3	−0·5
18°28′N, 66°7′W	8-11	26	5·1	26·5	460	5·1	·0	·4	1·26	4·0	
	12-15	26	7·0	49·3	670	5·1	−·1	·5	1·80	3·0	
	16-26	24	9·5	92·5	840	5·5	·2	·9	2·35	2·1	

* 1 knot = 0·515 m/sec † Standard deviation

It would be unwise, in such a preliminary analysis, either to put much weight on the apparent constancy of c with varying surface wind, or to suggest that the higher value for Johnson Island is meteorologically significant. With such small spread, however, in the values for each station it is obvious to seek an explanation of the higher Johnson Island values and two possible reasons come to mind. The first is topographical ; that the extensive reefs surrounding the island (see Table I) may produce a state of sea for which the aerodynamic as well as the geometric roughness is appreciably greater than for the ocean proper. This effect, however, might be expected to increase materially with increasing wind and of this the analysis shows no sign. Moreover if the reefs are sufficiently high the sea within them will be smoother than outside. The second suggestion is of a more fundamental kind ; that the drag is greater because of greater instability in the air. Table II gives the mean annual values of the temperature difference, air minus sea ($T_a - T_0$), obtained from *Monthly Meteorological Charts* M.O. 483, M.O. 484, and it will be seen that the Johnson Island region shows instability whereas the other two regions show stability. In any further study it will evidently be desirable to investigate this matter more fully. Information about the level to which the ' surface ' wind refers is not available, but it is unlikely that the high values of c can be ascribed to a variation in this quantity.

Coming now to the San Juan values, the apparent drag coefficient is seen to show a very marked, smooth, decrease with increasing surface wind, the minimum value being a little greater than the maximum for the Pacific Islands. The values, except perhaps the lowest, are almost certainly spurious, and evidence of topographical effects. These may be of two kinds, a systematic distortion of the wind field at the coast by the high land behind the coastal plain, possibly not of great importance, and systematic ageostrophic motion due to the diurnal variation of the temperature over the land.

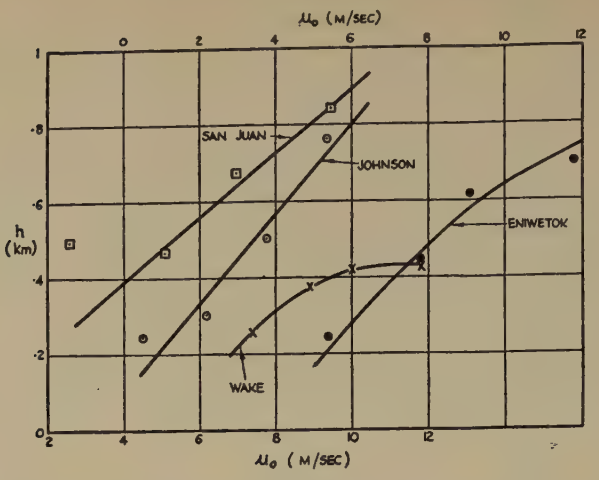

Figure 2. Variation of height h of maximum u with surface wind speed u_0 (lower scale San Juan and Johnson, upper scale Wake and Eniwetok).

The latter can be expected to diminish with increasing wind strength. It is worth noting that the instability of the air in the region of San Juan is greater than at any of the Pacific islands, for which reason the minimum value of c at San Juan, although high, may be quite near the true value for that area.

Two other features of the Trade-wind regime which are brought out by the present analysis are worthy of mention. The first is the practically linear increase of the level h of maximum u with surface wind speed u_0 (Fig. 2.), Wake being exceptional in showing a flattening-off in the relation for $u_0 > 6$ m/sec. Because of the small mean change of wind direction with height (see below) the level h may also be taken to be the level of maximum total wind speed within the error of evaluation. The variation of h shown in Fig. 2 is not quite a climatological average since soundings were selected in the present study on the basis of an upper limit to h (Section 2b) but it is evidently reasonably representative of conditions in the Trades. The smaller variation of h with u_0 for Wake is not to be ascribed to a lower limit having been placed on the individual values of h for this station ; the mean variation of h was practically unchanged when the selected soundings were increased to contain maxima up to the same level as for the other stations.

The level of maximum wind speed is controlled partly by surface friction and the turbulence accompanying it and partly by the direction and magnitude of the thermal wind and free-air turbulence. No solution of the equations of motion is available for this combination of circumstances and no analysis of the dependence, if any, of the thermal wind on the surface pressure field has yet been made, so that the relation of Fig. 2 must be left at present as it stands.

The other feature to be noted is the large inclination of the surface wind to the surface isobar and the quite small variation of the wind direction with height in the layer from the surface to the level of maximum wind. This is shown in Table III and is a reflection of the small contribution which v makes to the stress integral in Eq. (1) as already noticed.

TABLE III. MEAN ANGLE α_0 BETWEEN SURFACE WIND AND ISOBAR AND VARIATION OF MEAN WIND DIRECTION WITH HEIGHT. STATIONS SHOWN BY INITIAL LETTERS. GROUPS AS IN TABLE II. THE FIGURES IN PARENTHESIS ARE MEAN DEVIATIONS.

Wind group	α_0 (degrees) W	J	E	S.J.	Veer (deg) at Johnson Is. 0-1,000 ft	0-2,000 ft.
1	9 (17)	21 (16)	23 (15)	27 (19)	1·9	6·9
2	13 (12)	25 (10)	21 (9)	30 (10)	4·2	7·3
3	14 (13)	29 (14)	21 (13)	30 (13)	4·7	7·2
4	17 (10)	26 (16)	21 (6)	27 (8)	3·3	7·8

To a first approximation both α_0 and the change of wind direction with height, the latter evaluated for Johnson Island only, appear to be independent of surface-wind speed.

Note added in proof : The percentage ratio of the observed to the geostrophic wind speed at the surface for the stations examined above is available from the analysis and is as follows for the groups given in Table II : (1) 47, 58, 62, 67. (2) 45, 55, 63, 72. (3) 54, 58, 62, 73. (4) 32, 53, 65, 77. The increase of the ratio with wind speed is well-defined at all stations ; its magnitude will be discussed in a later paper. It must be remembered that the values are based on selected data.

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Precipitation streaks as a cause of radar upper bands

By I. C. BROWNE
*Cavendish Laboratory, Cambridge**

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SUMMARY

Several workers have reported enhanced radar echoes, from regions well above the freezing level, layer-type rain clouds. Usually, there is a decrease with time in the height of these ' upper bands,' Bowen has called such enhanced echoes. This paper describes an investigation into upper bands whi has been made at Cambridge, using a 3-cm radar with a vertically directed beam.

The mean downward velocity of upper bands at a height 9,000 ft above the freezing level is foun to be about 20 ft/sec, the velocity generally decreasing systematically with time. Such velocities a much greater than the rate of fall of the ice particles which are shown to be responsible for the upper-ban echoes, which, it is suggested, are produced by precipitation streaks in a region of vertical wind shear. simple theory is worked out which gives the apparent rate of fall of an upper band in terms of vertic wind shear, the horizontal wind speed near the cloud top, and the terminal velocity of the ice particl forming the precipitation streak. Measured values of the fall speed of upper bands agree reasonabl well with those predicted by this theory.

A previous theory of upper bands (Bowen 1951) is shown to be untenable, though it is possibl that the upper bands observed by Bowen in Australia have a different origin from those observed Cambridge. It is proposed to make further observations to settle this question.

1. INTRODUCTION

If the structure of precipitating clouds be investigated by radar, a plot of th echo intensity against the height of the echoing region may show, besides the well-define maximum from near the 0°C level (the melting band), subsidiary maxima from greate heights. These maxima retain their identity for periods of some minutes and, i general, apparently move downwards until the maximum merges with the meltin band. Such striations or maxima in the echo pattern were reported by Hooper an Kippax (1950a), and have been discussed in more detail by Bowen (1951), who terme them ' upper bands.' It will be convenient to use this term, although an interpretatio of the observations will be suggested which is very different from that of Bowen. It i also customary to speak of an upper band ' falling ' when what is meant is that ther is a systematic decrease in the height of the region from which the echo intensity i a maximum.

This paper gives the results of a detailed study of such upper bands at Cambridg in frontal rain. These bands have occurred occasionally in warm-front precipitatio and frequently in precipitation from cold-frontal conditions. The observations were made with a vertically directed beam (i.e., on overhead clouds), the echo patter being displayed as a plot of amplitude of echo signal against height of echoing region The radar wavelength was 3·2 cm. This arrangement gives high resolution of the vertical structure of the precipitating cloud and permits reliable measurements of the form and position of upper bands; it also makes possible reliable absolute measurement of echo intensity. It suffers from the disadvantage that only a small horizontal extent of the cloud can be studied at one time. The appearance of an upper band observed with this radar is shown in Fig. 1.

* Now at University of Manchester, Jodrell Bank Experimental Station.

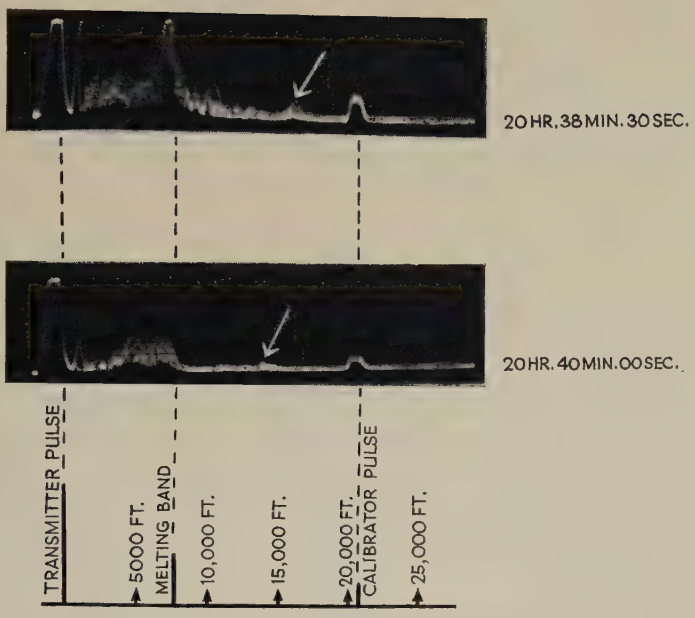

Figure 1. The appearance of upper bands. These photographs were taken at an interval of 90 sec during cold-frontal rain on 18 Oct. 1949. The rainfall rate at the ground was 2 mm/hr. The vertical axis represents echo amplitude and the horizontal axis, height of echoing region. The upper band is indicated by the arrows, to the left of which can be seen the melting band and transmitter pulse, while to the right can be seen the calibration pulse which shows that the receiver gain has been reduced in the second photograph. In fact, after correcting for the inverse-square law decrease of echo intensity with increasing range, the intensity of the upper band is the same in both pictures.

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2. Results of observations

In this section, the results of examination of more than a score of upper bands are summarized.

The apparent velocity of fall of the upper bands varies with height; in general, it lies between 10 ft/sec and 30 ft/sec when the band is about 9,000 ft above the melting band, the most common value being about 20 ft/sec. Usually the velocity decreases as the band falls, but it is difficult to measure the velocity accurately because upper bands often change in width and intensity in an erratic manner, the mean intensity being very little greater than that from above and below. Table I, for example, shows how the measured velocity of fall varied with time on two occasions. The velocities are averaged over the appropriate intervals of time.

TABLE I. Velocities of fall of two upper bands

(*a*)	21 Apr. 1949, 1615 hr				
	time (sec)	00	70	150	
	ht (ft) above freezing level	6,000	5,000	4,350	
	velocity (ft/sec)	14	8		
(*b*)	21 Apr. 1949, 1630 hr				
	time (sec)	00	35	95	140
	ht (ft) above freezing level	6,000	5,400	4,400	3,950
	velocity (ft/sec)	17	17	10	

Although the velocity of fall has sometimes remained constant, it has never been observed to increase with time, except when the bands pass through the melting band.

When an upper band reaches the melting band it merges with it temporarily, the intensity of the resultant melting band indicating that the upper band has intensified by a factor of about five as the particles which produce the band melt near the freezing level. Later, the intensity of the melting band decreases to its original value, while the echo intensity below increases slightly. It is not possible to distinguish a clear-cut band or maximum in this part of the echo pattern; often the whole of the rain echo intensifies simultaneously as though the upper band had broadened after its passage through the melting band. A minute or two afterwards, the rainfall rate at the ground usually increases by some 30 per cent for about a minute.

Bowen (1951) observed with a radar scanning through the zenith, and saw upper bands as roughly horizontal streaks of enhanced intensity which fell at speeds between 5·5 ft/sec and 7·5 ft/sec. The bands were reported to appear suddenly at heights corresponding to temperatures between $-14°C$ and $-18°C$.

3. Interpretation of observations

It is first necessary to ascertain whether the velocities of the upper bands are real, that is to say, whether the upper bands fall at the same speed as the particles producing them. (The identity of the particles producing a band may, of course,

be continually changing when the radar beam is vertical and stationary, and even wh it is scanning in a vertical plane.) Now the velocities possessed by the bands a those characteristic of raindrops and hailstones, and are too great for snow crysta and snow flakes, which fall at speeds between 2 ft/sec and 6 ft/sec. The observ enhancement of the melting band, as an upper band merges with it, is of the magnitu to be expected if the upper band is due to ice particles; while the subsequent effec indicate that these particles fall at about one-fifth of the speed of the raindrops whi they form when they melt, as in the ordinary theory of the melting band (Hoop and Kippax 1950b). For if they fell at the same speed as these drops, the intensi of an upper band, after it had fallen through the melting band, would be equal its maximum contribution to the echo intensity while the two bands were superpose because the concentration of particles would be the same above and below the meltin region. A detailed discussion of the melting band has been given elsewhere (Brown 1952). It is concluded that the particles producing the upper bands fall at a veloci of less than 3 ft/sec, and that the apparent velocity of fall of upper bands is not th velocity of fall of any echoing particle. Such a high apparent velocity could be produce by a slanting stream of precipitation particles drifting horizontally through the rada beam. Streams of this kind are often seen beneath distant shower clouds and a known variously as 'precipitation streaks,' 'rain streaks,' or 'fallstreifen.' Th slant of the streaks in this case could be produced by an increase of wind with heigh as the particles fall, they lag behind the region where they were formed, because the horizontal speed is always that of the wind in their immediate neighbourhood. It as though a man in a balloon drifting with the wind were to pour out water continuously The splash made by the water would move over the ground with the horizontal spee of the balloon, which equals that of the stream, even though the individual particle in the stream would move horizontally with the speed of the wind at their own heigh On this picture, the measured rate of fall of an upper band is the rate of descent of th point at which the curved precipitation streak is intercepted by the vertical rada beam.

We shall simplify the problem by supposing that the precipitation streak is bein generated at a certain point in the cloud. Take this point as the origin of coordinate z, measured vertically downwards, and x, measured horizontally into the wind. W start by finding the equation of the streak referred to these coordinates, and mak the important assumption that, over the region of interest, the wind speed W increase uniformly with height, so that

$$W = W_0 - kz$$

where k = vertical wind shear and W_0 = wind speed at the origin. Then it is easil shown that the equation for the precipitation streak referred to these coordinates, which are assumed to move with velocity W_0, is

$$x = \tfrac{1}{2}\left(\frac{k}{v}\right) z^2$$

where v is the vertical velocity of the particles in the stream. Thus the streak is parabolic in shape.

To find the apparent velocity of fall of the upper band produced by the streak, as it passes through the radar beam, we consider the streak to move with horizontal velocity W_0. Then at a time t after the origin of the streak passes through the beam,

We are looking at that part of the streak which has its x coordinate equal to $W_0 t$, or;

$$z = (2 v W_0 t/k)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and so the apparent velocity of fall of the band is :

$$u = (v W_0/2 k t)^{\frac{1}{2}}$$

so that u should decrease as $t^{-\frac{1}{2}}$.

Usually, the velocity which is measured is not the instantaneous velocity u, but an average velocity $\bar{u}$ defined by :

$$\bar{u} = (z_2 - z_1)/(t_2 - t_1)$$

$$\text{i.e.,} \quad \bar{u} = (2 v W_0/k)^{\frac{1}{2}} (t_1^{\frac{1}{2}} + t_2^{\frac{1}{2}})^{-1} \quad . \quad . \quad . \quad (2)$$

where t_1 and t_2 are measured from the moment of passage overhead of the effective origin of the streak.

In the case of the band shown in Fig. 1 (18 Oct. 1949), the measured value of $\bar{u}$ over a period of 90 sec was 22 ft sec^{-1}. The nearest upper air ascent (Downham Market, 21 hr) shows a wind shear of 2·9 kt/1,000 ft, (i.e., $k = 4{\cdot}9 \times 10^{-3}$ sec^{-1}), from 12,000 ft to above 20,000 ft. Assuming $v = 2{\cdot}3$ ft sec^{-1} (a value appropriate to moderate-sized ice crystals) and $W_0 = 80$ kt, the observed value at 20,000 ft, Eq. (2) gives the observed mean rate of fall if $t_1 = 25$ sec i.e., if the streak originated at a height about 17,000 ft above its position in the first photograph.

The observations on 21 Apr. 1949 (Table I) show some points of interest. The precipitation was associated with the passage of a cold front which was occluding. The height of the melting band shown by the radar was intermediate in value between the freezing levels ahead of and behind the front. The Downham Market ascent at 15 hr was clearly made before the front had arrived there; it shows a very small wind shear and it would be impossible to explain the observations from it. The next ascent, 21 hr, after the passage of the front, shows a pronounced wind shear ($k = 3{\cdot}9 \times 10^{-3}$ sec^{-1}) from 6,000 ft to 20,000 ft. Using this value of k, $W_0 = 68$ ft sec^{-1} (the value at 12,500 ft), and the same value of v as before, the following values of $\bar{u}$ are computed for the two sets of observations in Table I.

TABLE II. PREDICTED VELOCITIES OF FALL OF TWO UPPER BANDS, 21 APR. 1949.

(*a*) 1615 hr. The mean value of fall over the whole 150 sec requires the particles to originate 2,800 ft above the level of the first observation

Ht. above freezing level (ft)		6,000	5,000	4,350
Mean velocity of fall of band (ft/sec)	calculated	12·3	9·8	
	observed	14	8	

(*b*) 1630 hr. The mean rate of fall now requires the origin to be 1,800 ft above the level of the first observation

Ht. above freezing level (ft)		6,000	5,400	4,400	3,950
Mean velocity of fall of band (ft/sec)	calculated	18·9	14·0	11·3	
	observed	17	17	10	

It is not desired to stress either detailed agreement or disagreement betwee the observed and computed values from this oversimplified picture. The assumptic that an upper-air sounding some distance away in space and time gives appropria values for the wind shear in a raincloud at a cold front is a hazardous one, as is th neglect of vertical air currents. The computations demonstrate, however, that th theory does account for the behaviour of this type of radar band with values of win shear which are of the same order as those actually occurring in the atmosphere. Th examples chosen for discussion are taken at random and not specially selected to f a theory.

On this theory, the apparent broadening of an upper band after its passage throug the melting band is produced by the increased slope of the precipitation streak conse quent to the increase in terminal velocity v of the particles on melting. The theor also predicts that the measured thickness of the upper bands should decrease wit decreasing height. It has not been possible to test this prediction, owing to the lo intensity of upper bands at heights greater than about 9,000 ft above the meltin band.

It is obvious that upper bands due to precipitation streaks should move upwards not downwards, if the horizontal wind decreases with increasing height. Such a win structure may be responsible for the type of echo pattern which has often been observe towards the end of an overhead shower. After the rain has stopped at the groun the echo intensity decreases progressively upwards from the ground at an appare speed of about 30 ft/sec. It has been impossible to check whether the wind velocit on these occasions has, in fact, decreased with increasing height, because of the transien and local nature of the wind field around active shower clouds.

So far, we have not considered how the precipitation streaks are formed. A interesting observation may throw some light on this question. On one occasion (23 May 1951, 1430 hr), the rain was very light and the cloud from which it fell appeared not uniformly grey, but crossed in one region by a series of dark bands or striations apparently at a great height. The bands lay perpendicular to their direction o motion. As they passed overhead, a series of upper bands was observed with th radar, the bands appearing only while the striated part of the cloud was overhead. I would seem that this part of the cloud had a wavy top, the precipitation streaks being produced at the crests of the waves. Particles falling from the crests would fall further than those falling from the troughs, and so would grow more and return a stronger radar echo. The upper bands would then be given, not by ' streaks,' but by ' sheets of enhanced precipitation, as sketched in Fig. 2. If a radar scanning at right angle to the wind direction were used, upper bands similar to those reported by Bowen would be observed.

Bowen attempted to explain the formation of upper bands by the spontaneous freezing at about $-16°C$ of large supercooled droplets which have been carried to air at this temperature by updraughts, and by the subsequent rapid growth of the frozen droplets by the well-known Bergeron process of sublimation on to ice. There are many objections to this explanation. First, the echo intensity from the droplets would not increase, but decrease, as they froze. Secondly, the Bergeron process is too slow to enable the droplets to reach a terminal velocity several ft/sec greater than the updraught postulated by Bowen, who supposed the formation of an upper band to be completed in about 20 min. Thirdly, the fact that the temperature at the level where upper bands first appear is about $-16°C$ is not believed to be significant, since

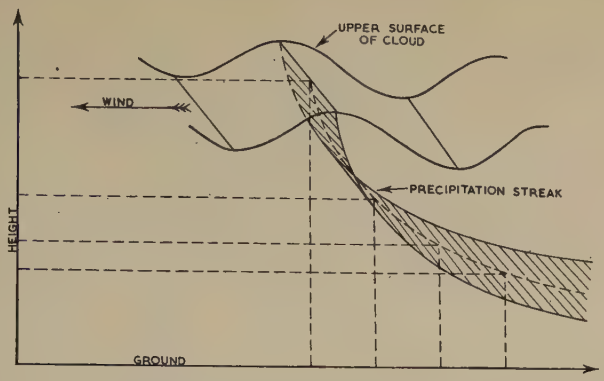

Figure 2. Sketch to illustrate the formation of an upper band. The pecked lines show how the height of an upper band varies with time.

in any case the radar echo is usually too weak to be detected at higher levels in clouds which are giving light, steady rain.

It is possible that the upper bands observed in Australia are different in kind from those observed at Cambridge, but if so it is curious that upper bands due to precipitation streaks have not been reported from Australia. Whether or not the phenomenon discussed by Bowen is in fact identical with that discussed in this paper may perhaps be decided conclusively by making simultaneous observations with both a stationary vertical beam and a radar scanning in a vertical plane. It is hoped shortly to make such observations at Cambridge.

Acknowledgments

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Measurements of condensation nuclei over the North Atlantic

By D. J. MOORE
Imperial College, London

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Summary

Measurements of the total concentration of condensation nuclei and of the concentration and si distribution of large sea-salt nuclei have been made on board an Ocean Weather Ship in the North Atlant over a period of three weeks. Total nucleus counts varied between 77/cm³ and 2,460/cm³, the numbe decreasing with increase in the intensity of vertical mixing as indicated by cloud type.

The concentration of large nuclei (mass $> 10^{-11}$ g) was found to increase markedly with wind spee and wave height, but no corresponding increase was found for the total nucleus population.

An analysis of the visibility observations at the Ocean Weather Ship Stations 'Item' and 'Jig' show that the opacity for a given humidity *increases* with wind speed and wave height and that this increa can be accounted for by the observed increase in the concentration of large nuclei.

Consequently if the rate of production of small nuclei at the sea surface increases with wind spee such increase (for wind speeds up to 18 m/sec) does not materially affect the total nucleus population few metres above the surface.

1. Introduction

The more recent researches on atmospheric condensation nuclei have recentl been reviewed by Mason and Ludlam (1951), and the evidence suggests that particle of oceanic origin may be an important source of these nuclei. The largest hygroscopi nuclei almost certainly consist of sea-salt, but whether nuclei can be produced i sufficient quantities from the oceans to account for global cloud formation is still a open question. Recent laboratory experiments by Facy (1951a, b) suggest that ver large numbers of small nuclei (probably of $MgCl_2$) may be produced by the burstin of foam bubbles in high winds while the original number of nuclei may be increase by the sea-salt crystals splitting into several fragments when the relative humidit falls below 50 per cent.

The most satisfactory way of determining the nature and origin of the nucle actually involved in cloud formation, would be to extend the work of Köhler (1922 and to collect droplets from non-precipitating clouds and determine the chemica nature of their nuclei. Comparison of these nuclei with the total nucleus populatio (as measured by an Aitken counter), would show what proportion of all aerosol particle actually become effective centres of condensation. This proportion no doubt varie considerably with meteorological and geographical conditions, so that a great numbe of observations would be required before the nature and origin of condensation nucle could be stated with confidence. Experimentally, the problem is a difficult one. The range of nucleus mass to be investigated is from 10^{-8} g down to 10^{-16} or even 10^{-17} g Nuclei smaller than 10^{-13} g cannot be examined under the optical microscope, anc even under the electron microscope the smallest (particularly the acidic) nuclei would not be capable of direct identification. The large range of nucleus size and of concentration means that different techniques for analysis and collection have to be employed in examining different parts of the nucleus spectrum.

As a modest contribution to this problem it was decided to make measurements of the total nucleus populations over the open ocean, remote from land sources ob

nuclei, and to obtain information on the size-distribution, concentration and nature of the largest nuclei. Although too rare to play a dominant role in cloud formation the latter may be very important in initiating shower production, especially in unstable maritime air masses. (Mason and Ludlam 1951, Ludlam 1951).

The observations were made on board the Ocean Weather Ship *Weather Observer* during September and October 1951 at two positions (1) 61° 20′N, 13° 20′W and (2) 59° 00′N, 19° 00′W, more than 200 mi from the nearest land. To avoid spurious results brought about by local sources of nuclei no observations were made when waves were breaking over the ship and all observations were made as far to windward of the funnel as possible.

2. Total concentration of nuclei

The total concentration of atmospheric condensation nuclei was measured with an Aitken counter when precipitation was absent. The results are shown in Table I. They may be compared with those obtained on the *Carnegie* (Landsberg 1938), where 40 per cent of the observations gave counts of less than 200/cm^3, 72 per cent less than 300/cm^3, and 87 per cent less than 1,000/cm^3, while Simpson (1941) quotes an average value of 940/cm^3 for all observations made over the oceans. Recent observations were made by Hess (1951) between New York and Le Havre (see also Parkinson 1952), the minimum count being 182/cm^3 and the maximum in mid-ocean, 2,500/cm^3. The average of all his counts for the eastbound journey was 956/cm^3 and for the westbound journey 1,019/cm^3. Average counts for winds of Force 5 and above were greater by about 150/cm^3 than those for lower wind speeds and he believed that salt nuclei from spray accounted for concentrations of from 100/cm^3 to 500/cm^3 over the open sea.

TABLE I. Total concentrations of nuclei at o.w.s. station 'Item' Sept./Oct. 1951

Concentration (per cm^3)	100	101-200	201-500	501-1,000	> 1,000
No. of occasions	2	5	6	8	6

Average concentration = 703/cm^3. Max. = 2,460/cm^3. Min. = 77/cm^3.

The close association between the total concentration of nuclei and the intensity of vertical mixing is shown in Table II where the nucleus counts are grouped according to cloud type. It will be seen that the smallest nucleus populations existed under conditions of convective activity (Cu or Cb), highest counts with stratus cloud and intermediate concentrations with stratocumulus or a mixture of cumulus and stratocumulus. The association appears not only in the average values, but also in the maximum and minimum counts.

The last two lines of Table II show that the average concentrations were rather lower for each cloud type when the wind was blowing from the open sea, and consequently higher when it was blowing from the nearest land, some 200 mi away. From this it appears that land sources can influence the nucleus concentrations at these distances.

No significant correlation was found between the *total* nucleus populations and either wind speed or wave height.

TABLE II. TOTAL NUCLEUS CONCENTRATIONS GROUPED ACCORDING TO CLOUD TYPE

Cloud type	St	Sc or Sc and Cu	Cu or Cb
No. of observations	7	11	9
Average concentration (cm^{-3})	1,280	625	354
Highest concentration	2,460	1,490	740
Lowest concentration	290	132	77
Average concentration with wind from open sea (165°-280° position 1, 155°-360° position 2)	715 (3)	386 (6)	260 (6)
Average concentration with wind from other directions	1,680 (4)	917 (5)	544 (3)

(No. of observations in parentheses)

3. MEASUREMENTS ON LARGE NUCLEI

Large nuclei (mass $> 10^{-11}$ g) consisting of droplets of sea spray were caught on glass slides or rods coated with magnesium oxide, the diameters of the drops being deduced from measurements of the craters produced in the oxide film. The mass of salt in each drop was calculated on the assumption that at the moment of impact each drop consisted of a solution of sea-salt saturated with sodium chloride (see Table III). Occasions of relative humidity less than 80 per cent only were considered. The nucleus samples were taken at a height of $1\frac{1}{2}$ m above deck level ($\simeq$ 6 m above the water line) at times when there was little chance of spray produced by the ship reaching the collectors. That the droplets contained sea-salt (or at least NaCl), was confirmed in some cases by the appearance of well-defined cubical crystals in the craters.

TABLE III. TIME FOR AN UNVENTILATED SPRAY DROP TO EVAPORATE TO A SOLUTION SATURATED WITH NaCl IN AN ATMOSPHERE OF 70 PER CENT RELATIVE HUMIDITY AT 10°C

Salt content (g)	10^{-8}	10^{-9}	10^{-10}	10^{-11}
Evaporation time (sec)	36	7·8	1·7	0·36

The radius of the largest drop is 41 μ immediately after it is formed, so the effect of ventilation would not be important in the range of nucleus mass considered (Kinzer and Gunn 1951).

The glass collectors were microscope slides 2·5 cm wide and rods about 200 μ in diameter. The former have a collection efficiency of 0·7 for nuclei of radius 15 μ with wind speeds down to 7 m/sec while the latter can collect droplets of 2 μ radius with the same efficiency down to about 2 m/sec (Langmuir and Blodgett 1946). Examples of the spread of nucleus mass obtained at two different wind speeds when the collectors were exposed simultaneously are shown in Fig. 1 and may be compared with the results obtained by Woodcock (1950) at Hillsboro Lighthouse and by Fournier D'Albe (1951) over the Bay of Monaco.

Fig. 2 shows the concentration of large nuclei (mass $> 10^{-11}$ g) to increase at higher wind speeds, an effect which was also found by the two last-mentioned authors. A remarkable relationship was found between the logarithm of the concentration of the *giant* nuclei (mass $> 5 \times 10^{-9}$ g) and the height of the waves (see Fig. 3), while there was a tendency for their concentration relative to that of nuclei of mass $> 10^{-11}$ g to

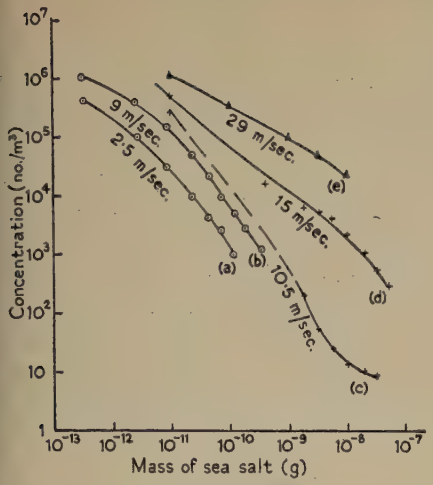

Figure 1. Concentrations of nuclei of mass $> m$. Curves (a) and (b) after Fournier D'Albe, (c) and (d) observations made at 'Item' and (e) after Woodcock.

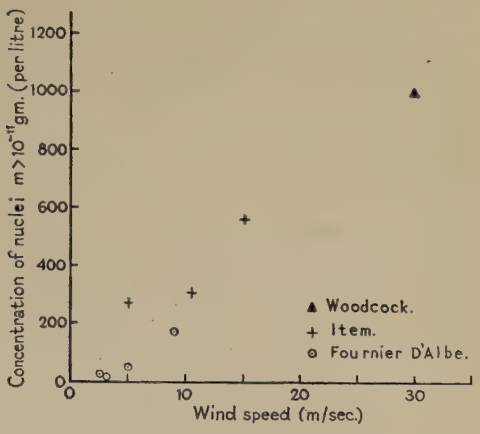

Figure 2. Concentrations of nuclei of mass $> 10^{-11}$ g as a function of the wind speed.

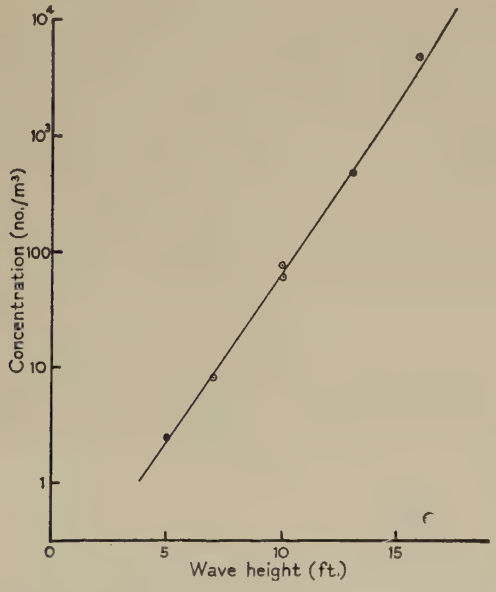

Figure 3. Concentrations of nuclei of mass $> 5 \times 10^{-9}$ g (radius $> 15\ \mu$ on collection) as a function of the wave height.

increase markedly with increasing wind speed, which is in agreement with similar observations made by the author near Dover.

That there was such a strong correlation between the concentration of the large sea-salt nuclei and wind speed and wave height, but no such correlation in the case of the total nucleus concentration as measured by the Aitken counter, suggests either that most of the smaller nuclei were not of oceanic origin, or that any increased production of the smaller nuclei in high winds was largely counterbalanced by their enhanced upward transport from the sea surface.

4. Contribution of the large nuclei to atmospheric opacity

The opacity of the atmosphere is determined by the size and concentration of the suspended particles and the relative humidity which controls the size of the hygroscopic particles. Wright (1939) analysed the observations of visibility taken over a number of years at various stations in the British Isles and calculated mean values of the opacity over different ranges of relative humidity. Assuming that, over a long period, the average number of nuclei was independent of humidity, an assumption there is some reason to doubt, he ascribed the change of visibility solely to the change of nuclear size with relative humidity and estimated the average concentration of nuclei at Valentia to be 63/cm^3 and the average nucleus to contain $1{\cdot}1 \times 10^{-13}$ g of sea-salt. In a recent paper, Yamamoto and Muira (1951) have given the following correlation coefficients for monthly mean values of opacity, humidity and wind speed at Sendai (Japan) :

> Opacity and humidity + 0·829, opacity and wind speed — 0·477, humidity and wind speed — 0·837.

In order to estimate the contribution which nuclei of oceanic origin might make to the visibility over the open ocean, and in view of the strong correlation between the concentration of large nuclei and the wind speed mentioned in Section 3, it was decided to investigate how the visibility (or opacity) as measured on the Weather Ships varied with the wind speed. In view of an expected large negative correlation between wind speed and humidity it appeared desirable to fix one of the three variables and look for a relationship between the other two. Accordingly, observations of visibility made daily at 12 GMT at Weather Ship Stations ' Item ' and ' Jig ' from 1947 onwards were grouped according to values of the relative humidity. As visibility is largely independent of relative humidity when the latter is less than 70 per cent all observations in this group were taken together, the rest being grouped over five per cent intervals in relative humidity. Occasions of fog, precipitation, or precipitation within the last hour were excluded.

TABLE IV. Droplet opacity as a function of wind speed for observations at ' Item ' and ' Jig ' for the period 1947-1951 at relative humidities less than 75 per cent

Wind force (Beaufort scale)	0-1	2	3	4	5	6	7	8	9-10
Droplet opacity (neb/km)	1·9	1·5	1·7	1·7	1·9	2·2	2·1	2·6	3·8
Number of observations	43	45	80	154	116	115	49	26	12

If the opacity is n nebules/km then $\sigma = 0.069\,n$, where **a** is the extinction coefficient (km^{-1}). In ideal circumstances for an observer with normal vision, $n = 57/v$, where v is the visual range in km (see Wright (1939) for a full discussion).

Table IV shows values of ' droplet opacity,' i.e., total opacity less the contribution owing to molecular scattering taken as 0·23 neb/km against wind speed on the occasions when the humidity was less than 75 per cent. The droplet opacity appears to remain sensibly constant at about 1·6 neb/km, for wind speeds up to 7 m/sec, after which it increases with increasing wind speed to about double this value with winds of 20 m/sec (at higher wind speeds the effect of large spray drops on visibility becomes important). Table IV indicates that, on average, the opacity increases by about

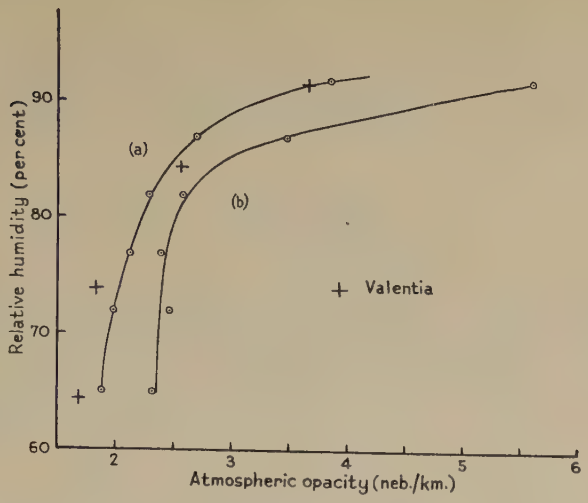

Figure 4. Variation of opacity with relative humidity at 'Item' and 'Jig' for (a) wind force 0-4 and (b) 5-7.

0·7 neb/km at low humidities as the wind speed increases from 7·5 m/sec to 15 m/sec. Observations made by the author indicate that such an increase may be accounted for by the greater numbers of large nuclei present at the higher wind speeds, although more observations will have to be made to establish this point. It appears therefore that these changes in opacity do not indicate appreciably larger concentrations of the smaller nuclei near the sea surface and so lend support to the conclusions reached in Section 3.

In Fig. 4 the relationship between opacity and humidity is shown for the occasions when the wind force was (*a*) 0 to 4 and (*b*) 5 to 7. An increase of opacity with wind speed is noted at all humidities, which is at variance with the findings of other workers (e.g., Yamamoto and Muira) who did not separate from their data the negative correlation between wind speed and humidity.

Curve (*a*) of Fig. 4 showing the relation between opacity and humidity for low wind speeds agrees fairly well with Wright's (1940) analysis of the Valentia observations for winds blowing off the ocean, indicating that such a curve represents the average residual opacity for the eastern North Atlantic. The horizontal displacement between curves (*a*) and (*b*) is a minimum at about 80 per cent relative humidity indicating that at lower humidities the increase of opacity with wind speed is more marked when the air is drier. Such a result might be attributed to the enhanced production of nuclei at low relative humidities by the dehydration of larger droplets. However, it must be remembered that observations of visibility made on board Weather Ships are little more than intelligent guesses, so it would be unwise to put much weight on the actual magnitude of the calculated increases in opacity, e.g., the differences in opacity of the first four values of Table IV are probably of no significance.

Acknowledgments

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Some further aspects of night cooling under clear skies

By W. E. SAUNDERS
Meteorological Office, London

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Summary

The nature and cause of the evening temperature discontinuity near the ground is discussed, evidence being given that this is a result of the commencement of dew formation. Regression equations for forecasting the temperature of discontinuity are given. Methods of forecasting the subsequent cooling are discussed, and a detailed procedure given for Northolt. The relation between screen and grass minimum temperatures is briefly discussed.

1. Introduction

Detailed forecasting of night cooling is an essential requirement in aviation forecasting, where it is necessary to estimate not only whether radiation fog will form but at what time the screen-level temperature of fog formation (the fog point) will be reached. In industry too, particularly for electricity undertakings, detailed forecasts of night cooling are increasingly required. A forecast of the night minimum has many other applications, even where greater detail is not needed.

It is a fact of observation that the night-cooling curve is typically not a smooth curve but is characterized by a steep decline in the evening followed by a much more gradual decline, the two phases being separated by a discontinuity. The fact was taken into account at some stations during the war years, and is by now probably familiar to most forecasters. It has already been discussed by the writer (Saunders 1949, referred to below as I).

The present approach is first to forecast the time and temperature of the discontinuity, then to estimate the night minimum as the further depression from this value. In this way a fair fix on the whole curve is obtained, and the temperature at any time can be interpolated.

2. Time and temperature of the discontinuity

As further evidence of the reality of the discontinuity in cooling, attention is called to a 1948/9 paper of the University of Texas. Half-hourly readings of temperature are given for various levels above and below the ground, from which it is apparent that the discontinuity is pronounced very near the ground, less so at screen level, delayed by about an hour at a depth of 2 in, and difficult to recognize above and below. An example is given in Fig. 1. It appears then that the phenomenon is primarily a feature of the boundary layer, which supports the suggestion, made independently to the author by S. F. Witcombe and J. M. Craddock, that the discontinuity might mark the time of first deposit of dew, the latent heat release retarding the temperature fall. This interpretation is supported by a few simple experiments made by the writer on seven clear-sky, light-wind evenings in June 1950. Over a closely-mown lawn dry- and wet-bulb thermometers were set up at a height of 1 in, and frequent readings taken. In every case, somewhere between 2000 and 2010 hr, the time of saturation and reduced rate of cooling coincided with an obvious and sudden

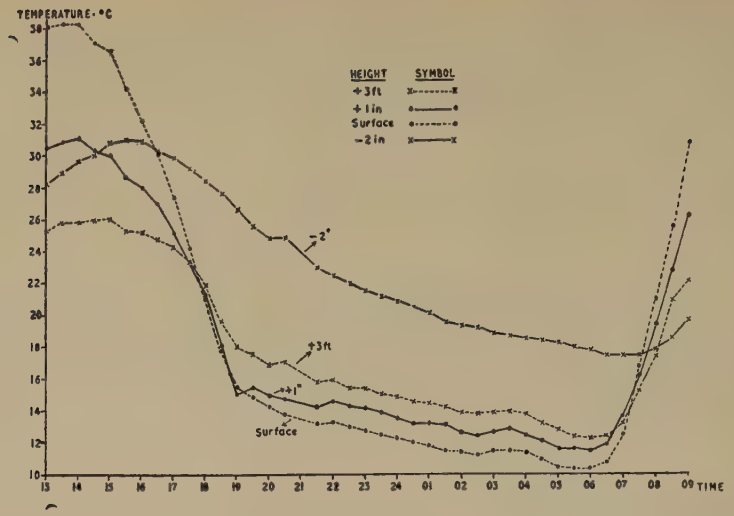

Figure 1. Air and soil temperatures at Manor, Texas, for 27-28 Sept. 1948 (University of Texas Report).

formation of heavy dew. The thermometers were screened from direct radiation with aluminium foil. There are some unresolved difficulties. On three occasions the deposit coincided with a sudden slight rise in temperature, and this is known to occur from time to time at screen level. Also the discontinuity occurs on more windy nights, when there is no visible dew deposit on the grass. On these occasions condensation may take place on the ground, where turbulence is much suppressed by the grass. It may be suggested that the phenomenon is due to a sudden increase in turbulence in the boundary layer, but this could not bring moisture downwards unless there were an inversion of the hydrolapse, and it is not easy to see how this could be set up without the *previous* deposit of dew. On the whole the evidence seems to support the view that the deposit of dew is a cause and not an effect of the discontinuity in cooling, but there is clearly a need for more experimental work.

The phenomenon was further studied from screen temperatures at Northolt taken at half-hourly intervals through the period August 1948 to April 1950. Having eliminated cases where the discontinuity might have been due to passing cloud, or to effects such as the termination of a sea breeze, or the commencement of a katabatic flow, which are liable to produce a similar break in the cooling, the time and temperature of the discontinuity were noted for each clear evening. The times are represented by the crosses in Fig. 2 and, as some indication of the soil moisture, the daily rainfall is shown by ordinate lines. The scatter is partly due to the times having been taken from half-hourly reports, not from thermograms.

As pointed out in I, the time of discontinuity appears to be unaffected by wind speed. The reason for the time regularity is believed to be that whatever the ' surface '-wind speed (measured conventionally), turbulence is so reduced, and cooling so rapid, within the grass, that saturation is reached at very nearly the same time each evening. Cooling is that of the ground surface itself, and the time varies with sunset and with nature and state of ground, independently of air temperature and humidity. During a short period in which the state of ground does not vary appreciably, as in the writer's experiments in June 1950, the time remains practically constant from night to night.

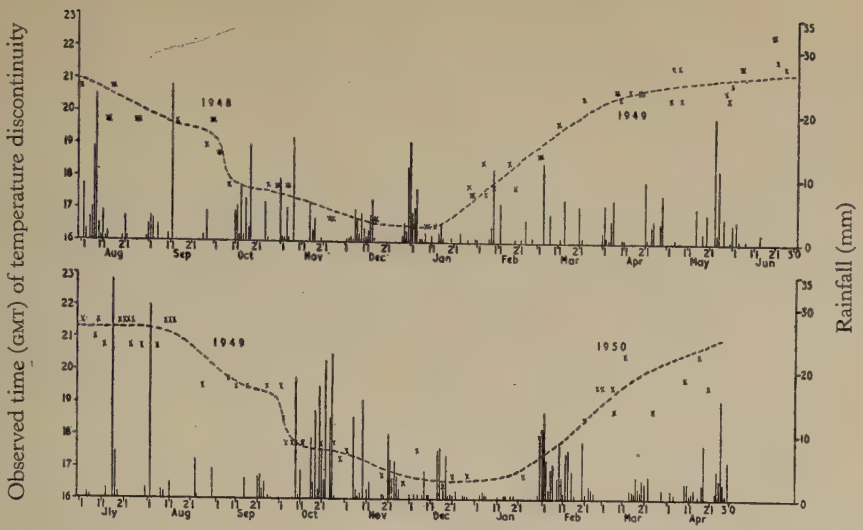

Figure 2. Variation of observed time of evening temperature discontinuity with time of year and with preceding rainfall (times of discontinuity marked x, rainfall given by ordinate lines)

In both years there is a sudden change in the time during the first week in October, and similar changes have been noted in other years. In spite of the fact that this precedes the main rainfall it is believed to mark the transition to the winter period when the grass remains permanently moist (initially perhaps, in the absence of rain, owing to dew which fails to evaporate), when relative humidity in the near-surface layer is always high, and when dew forms very readily. During the winter, rainfall does not appear to affect the time, but in summer there is an indication, e.g., in May-June 1949, that the time is earlier after rain. This effect is distinctly more pronounced in spring and in early summer than in late summer. The monthly average times are in fair agreement with those given previously for Abingdon. They agree well with the times for Bovingdon. The times for London Airport and Croydon are systematically later owing to soil differences.

In I a method of forecasting T_τ (screen temperature at change of cooling rate) from the tephigram was described. This gave satisfactory results during a three-summer-months trial period at Abingdon, when it was usually possible to forecast T_τ within $\pm$ 1°F. It was however later found that this method failed on certain occasions, mainly in winter, when a temperature inversion near the ground failed to break down during the day. Now that more is known of the nature and origin of the discontinuity it appears more logical to use the dew point, and, following a method used in another connection by Craddock (1951), we have obtained regression equations for forecasting T_τ in terms of T_{max} (maximum temperature) and T_d (dew point at time of T_{max}). It was found by inspection that T_τ also depends on the afternoon temperature lapse rate, being significantly lower in cases of stability, higher with a steeper lapse. By separating the cases according as there was or was not an afternoon inversion with base at or below 900 mb quite small scatter was obtained about the regression lines. The formulae are :

with no inversion $T_\tau = \frac{1}{2}(T_{max} + T_d) - 0{\cdot}6$°F (78 occasions, s.d., 1·04°F)

with inversion $T_\tau = \frac{1}{2}(T_{max} + T_d) - 4{\cdot}0$°F (27 occasions, s.d., 0·88°F)

To have followed Craddock's method completely would have involved correlating T_T with $T_{max} - T_d$ as well as with $\frac{1}{2}(T_{max} + T_d)$, but no significant relation between these variables could be found, and for practical purposes the simplicity of the equation found is a useful feature.

This method is rather easier to apply than that previously proposed, where it was sometimes difficult to say to what height diurnal heating had reached on the tephigram, and it has the advantage that allowance may readily be made for advective changes, where necessary, by taking T_{max} and T_d upwind of the stations whose T_T is being forecast. Although in a few inversion cases both methods appear to give a satisfactory result for T_τ, e.g., in the example given in Fig. 5 of I, where the tephigram gave the correct T_τ in accord with the thermogram discontinuity, while T_{max} and T_d were 64°F, and 42°F, so that the appropriate regression equation would have given similar accuracy – there seems no doubt that the later method, which has been tested over a longer period, is the more accurate.

As regards the variation of T_τ with the afternoon lapse rate, possible causes are, (*a*) an inversion commonly brings very dry air near the ground, and on these occasions the surface dew point is less representative of the humidity than in the non-inversion case. But if this were the cause we should expect variations in T_τ to be more random, whereas in fact the regression for T_τ is slightly more accurate in the inversion case. Perhaps a more likely explanation is, (*b*) greater cooling is permitted by the normal suppression of turbulence beneath the inversion.

3. Subsequent cooling

It must be expected that subsequent cooling will be affected by many factors, of which the nature and state of soil (conductivity, diffusivity, specific heat), characteristics of air mass (temperature and humidity profiles, probably haze content), nature of condensation (dew or hoar frost), and wind speed, are certainly important variables. There is little prospect of being able to deal with all these in theory. The practical problem is to select forecasting parameters which will represent as many of them as possible.

In the present analysis the cases were first separated into those in which there was an effective calm at the screen (' surface '-wind calm or very light), and those in which mixing obviously extended down to this level. The precise demarcation was finally made in terms of the mean gradient wind speed, and was found by inspection.

In the case of calms, during the period of subsequent cooling, the fall of temperature at the screen is practically identical with that at the ground (e.g., see Fig. 1), which leads to some simplification of the problem. The denominator of Brunt's (1939) expression for the fall of temperature at the ground (the product $\rho_1 c_1 \kappa_1^{\frac{1}{2}}$) varies appreciably with the nature and state of the soil. In this work we are considering only one soil, London clay, and we allow for state of soil by dividing the year according as the ground is normally dry or wet, and by providing for wet or dry ground during summer. It then appears that one parameter is needed to represent the net outgoing radiation, and another to represent the time available for cooling. The net radiative heat loss decreases with decreasing temperature. In a recent unpublished memorandum Craddock has shown that it decreases by about $2\frac{1}{2}$ per cent for each 1°F fall in surface temperature. This was based on a case quoted by P. Groen (1947), where it was shown that a net heat loss of 9 cal cm^{-2} hr^{-1} at sunset was reduced to 4 cal cm^{-2} hr^{-1}

to 5 cal cm^{-2} hr^{-1} after the temperature had fallen by 12°C. It follows that the most suitable parameter to represent the net radiative heat loss will be the temperature itself. Temperature as a parameter has the further advantage that it gives some representation of the latent heat difference between dew and hoar frost – the higher the initial temperature the less likelihood of cooling being reduced by the greater latent heat release for hoar frost than for dew.

With stronger winds screen-level cooling is retarded by downward diffusion of heat. The number of degrees fall of temperature is proportional to the coefficient of eddy diffusivity of heat, and so to the wind speed. This has been confirmed by recent work in Tokyo by Ogura (1949).

Following on these considerations, the night minimum for clear nights was derived in the following ways :

(*a*) With calm and light winds T_{min} was expressed as a function of the calculated value of T_r, separately for summer and winter half-years (i.e., temperature was regarded as the main variable during periods in which the state of top soil is unchanged, except after rain). Allowance was made for the decreased length of time available for subsequent cooling during the midsummer period ; (*b*) with stronger winds the fall of temperature was expressed as a function of the mean gradient wind speed.

Clear nights were regarded as those in which the mean cloud amount, excluding broken cirrus, did not exceed an eighth. Occurrence of fog with a ' sky obscured ' report was regarded as cloud and omitted. Cases included form 75 per cent of the total clear nights during the period. In the remainder, behaviour of the Northolt dew point gave clear indication of advective change. In this basic work it was not considered valid to select upwind values of T_{max} and T_d in these cases, so they were omitted.

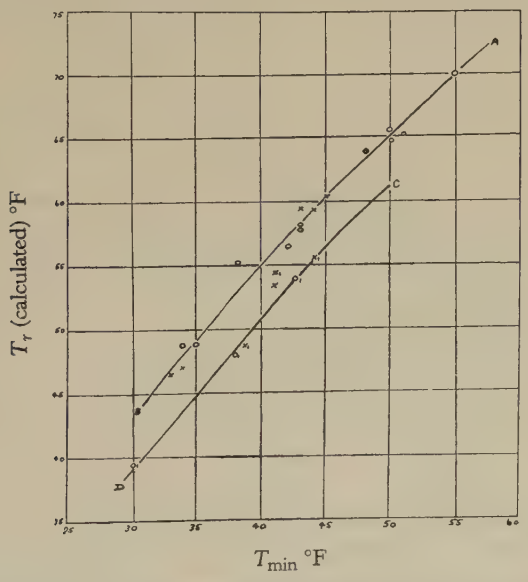

Figure 3. The relation between initial and final temperatures of the period of subsequent cooling during the spring and late summer for occasions of calm (mean gradient wind 0-14 mi/hr) and light wind (mean gradient wind 15-21 mi/hr). Cases which were ' non-inversion ' for calculation of T_r are marked o if the mean gradient wind was 0-14 mi/hr, and o_1 if the mean gradient wind was 15-21 mi/hr. Cases which were ' inversion ' for calculation of T_r are marked x if the gradient wind was 0-14 mi/hr, and x_1 if the gradient wind was 15-21 mi/hr. AB and CD are the curves drawn to fit cases o x and o_1 x_1 respectively.

The results are presented in graphical form in Figs. 3, 4 and 6. Fig. 3 gives the relation for calm and light winds for spring and late summer, taken as 25 March to 4 May and 11 Aug. to 7 Oct. Fig. 4 gives the corresponding relations for winter.

The transitional dates were chosen independently to give the best fit, but are clearly related with the change of season indicated in Fig. 2, and so, as we have surmised, with the state of ground.

The separation into days of inversion and no inversion introduced into Section 2 was carried forward into this analysis, the observations being represented by circles and crosses. In summer, afternoon stability has no obvious effect on subsequent cooling, but in winter there is a tendency for cooling to be greater in the stabler air mass (Fig. 4, AB compared with CD). A separation by wind speed was also adopted.

It was found necessary to apply a time correction during the midsummer part of the summer half-year, when the period of subsequent cooling falls below about eight hours. The amount of cooling indicated by Fig. 3 is reduced to the extent indicated by Fig. 5 (i.e., varying with the date and with the wind speed), provided the top soil is dry. This shows that subsequent cooling is reduced from about 15°F in April and September to about 12°F in late June. This difference is readily predictable from Brunt's equation, which shows that, with other factors unchanged, the number of degrees cooling is proportional to $t^{\frac{1}{2}}$, where t is the cooling period. The subsequent cooling is 15°F in April, and again in late August and September, when the period averages 9 hours. In late June, when the period is 6 hours, the subsequent cooling is reduced to $15 \times (6/9)^{\frac{1}{2}} = 12$°F approx. The time correction clearly applies to the period of subsequent cooling, not to total length of night. After rain T_τ is reached early, and subsequent cooling tends towards the full 15°F. There is no indication of a corresponding time correction during winter, when probably cooling becomes so slow towards the end of the night that time variations do not affect the result.

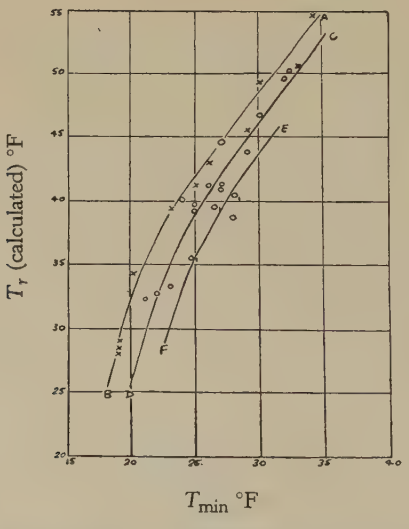

Figure 4. The relation between initial and final temperatures of the period of subsequent cooling during the winter for occasions of calm (mean gradient wind 0-19 mi/hr) and light wind (mean gradient wind 19-24 mi/hr). Cases which were ' non-inversion ' for calculation of T_τ are marked o if the mean gradient wind was 0-19 mi/hr, and o_1 if the mean gradient wind was 19-24 mi/hr. Cases which were ' inversion ' for the calculation of T_τ are marked x if the mean gradient wind was 0-19 mi/hr. AB, CD and EF are the curves drawn to fit cases x, o and o_1 respectively.

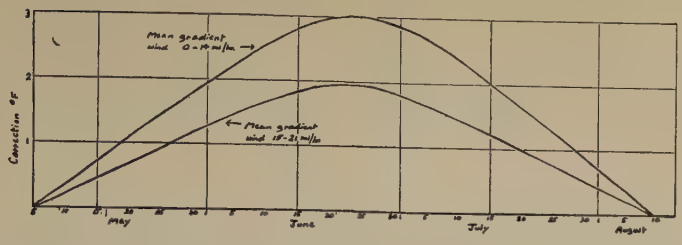

Figure 5. Correction for reduced length of period of subsequent cooling when Fig. 3 is used during the midsummer period (note that if T_r is reached early owing to moist top soil this correction will be partially or wholly offset).

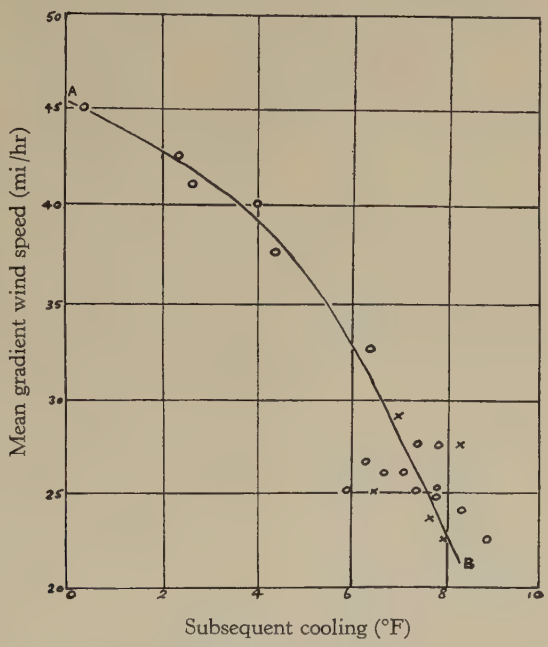

Figure 6. The relation between the number of degrees subsequent cooling and the mean gradient wind speed for occasions of stronger wind (lower limit of gradient wind speed 22 mi/hr in summer and 25 mi/hr in winter). Cases which were non-inversion for calculation of T_r are marked o and 'inversion' cases x. AB is the curve drawn to fit the cases plotted.

Fig. 4 suggests that, however low T_r, T_{min} would not fall below 16°F to 17°F but the analysis does not include cases of snow for which a lower minimum is to be expected. Fig. 6 gives the relation for stronger winds. Evidently the correlation increases with wind speed.

4. Application to forecasting at Northolt

The forecasting procedure at Northolt is :

(*a*) Take the observed T_{max} and T_d for the afternoon. Consider if they are representative of the air likely to be over the locality during the night. If not, make a judgment from observations upwind.

(*b*) Estimate synoptically, allowing for trend and advection, whether or not there will be an inversion at or below 900 mb.

(*c*) Calculate T_r from the appropriate regression equation.

(*d*) Estimate the time of T_r from Fig. 2, subtracting 1 hr in late spring and earl summer, and $\frac{1}{2}$ hr in late summer for wet top soil, if necessary.

(*e*) From the forecast T_r and the forecast mean gradient wind speed use Table to obtain T_{min} :

TABLE I.

Season	Forecast mean gradient wind mi/hr	Method for T_{min}
Summer (late March to early October)	0-14	Fig. 3, curve AB } *Add* to T_{min} a time correctio
	15-21	Fig. 3, curve CD } from Fig. 5 during 5 May t 10 Aug., except after rain.
	over 21	By $T_{min} = T_r$ (calculated) $- \Delta$ °F where Δ given b Fig. 6.
Winter (early October to late March)	0-19	Fig. 4, AB or CD according to the afternoon lapse rat
	20-24	Fig. 4, EF.
	over 24	As in summer.

5. Application to forecasting at other stations

This approach to the practical problem provides no simple all-purpose formul but requires care and judgment. Forecasters do not however expect to find simpl answers to complex problems, and will not object to the procedure if it provides usefu results. Some notes are now given regarding differences which may be expecte at other stations.

First in connection with the time of discontinuity, a change in rate comparabl with that discussed in the present paper may occur at any time owing to other cause than the commencement of dew – cf. Section 2. Secondly it has already been shown in Section 2 that there are systematic time differences in the London area. It is thought that the times given for Northolt on Fig. 2 will be applicable to clay soils elsewhere, but that on other soils the times will differ slightly.

As regards the methods for T_{min}, a comparison between Northolt and London Airport on 25 clear, calm nights showed that T_{min} was always higher at London Airport, where there is no clay near the surface. The mean difference was as much as 4·0°F (s.d., 1·5°F). The mean T_{max} on the previous afternoon was only 0·5°F higher at London Airport, so that the difference in T_{min} must be attributed almost solely to differences in soil and exposure. The difference in T_{min} decreases as the wind speed increases. Clearly therefore, in applying these methods at stations on other types of soil, separate corrections must be deduced from a sufficient number of cases for each section of Table I. This is especially the case with Figs. 3 and 4. With Fig. 6 differences may be quite small.

In the case of coastal stations the present technique can of course only be expected to apply with wind calm or off the land. With wind off the sea different methods must be applied to allow for the effect of sea temperature.

6. Accuracy of the methods for T_{min}

The mean square error derived by measuring the scatter of individual cases from the fitted curves on Fig. 3, 4 and 6 was 0·98°F. As the number of cases on some

parts of the curves was small the methods described above were tested against the Northolt observations for a further period, April to July, 1951. Of 34 clear nights there were 4 in which the dew point gave clear indication of advective change. In the remainder the mean square error between forecast and actual T_{min} was 1·28°F, a slight decrease in accuracy resulting from the complication of the midsummer time correction.

The accuracy in deducting T_{min} from T_r (calculated) is therefore about the same as that for obtaining T_r (actual) from the regression equations. This is probably due to the fact that T_{min} is a measured quantity, whereas T_r (actual) was interpolated from half-hourly temperatures. Since Smith (1951) has shown that a tolerance of about $\pm$ 1°F may have to be allowed in the measured dry-bulb and dew-point temperatures we may perhaps conclude that the main variables have been taken into account.

The attainment of this order of accuracy in forecasting practice depends, of course, upon an accurate forecast of the cloud amount and wind speed. On the other hand only Northolt values of T_{max} and T_d were used. These were not always necessarily the most representative for the night, and the forecaster usually has a number of values upwind from which to choose.

7. The grass minimum temperature

The relation between the screen minimum (T_{min}) and the grass minimum (T_g) temperatures was investigated for the same 1948-1950 period, as shown in Tables II and III.

TABLE II. Variation of difference between screen and grass minimum temperatures with the wind speed

Season	Gradient wind speed mi/hr	Mean $T_{min} - T_g$ °F	s.d. °F
Summer	0-14	8·0	1·5
Winter	0-19	(varying with temperature in accord with Table III)	
Summer	15-28	11·1	2·5
Winter	20-28		
Whole year	over 28	7·4	1·3

TABLE III. Variation of difference between screen and grass minimum temperatures with temperature for occasions of calm and light winds during the winter

T_{min} °F	Mean $T_{min} - T_g$ °F
31-35	4
26-30	7
21-25	9
16-20	11

Table II shows that the first effect of increasing wind speed is to increase $T_{min} - T_g$, corresponding with the stage at which turbulent mixing with warmer air extends down to screen level, but not so effectively to the grass. When the gradient wind exceeds 28 mi/hr mixing evidently does extend to the grass. The rapid increase in $T_{min} - T_g$

with decreasing T_{min} on calm winter nights (Table III) is no doubt due to the decreasin amount of moisture available for forming hoar frost, and so of latent heat released. T_{min} is, say, 34°F, $T_{min} - T_g$ will be 8°F in summer with dry soil, but only 4°F winter when the soil is permanently moist.

8. Conclusion

The results show that reasonable accuracy in forecasting night cooling is possibl if sufficient parameters are used, and that this can be done without use of absolu values of the radiation, or of the soil ' constants.'

The results for Northolt require verification on other clay soils, and curves fo other soil types could be deduced. Corrections for use on overcast and partly cloud nights can be tabulated. Observations are also needed to determine, for various soil the variations in time of discontinuity, and in transitional dates for summer and winte curves in relation to moisture content.

Acknowledgments

The author is indebted to the Director of the Meteorological Office for permissio to publish this paper, also to Dr. R. C. Sutcliffe for advice at various stages, to M G. A. Corby for many useful discussions, to Mr. S. F. Witcombe and Mr. J. M. Craddoc for suggestions made, and to Mr. F. W. Hitchings for help with the diagrams.

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The profile of a jet stream observed 18 January 1952

By G. W. HURST
Meteorological Office, London

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In the afternoon of 18 January 1952 a well-marked jet stream covered the western half of Great Britain, and winds were almost due northerly at all heights. Conditions over the British Isles were those of small well-broken cumulus (except in the extreme east where there were showers) with scattered cirrus, and there was a warm front at about 20°W. moving east rather slowly, with a feeble ridge to the west of Ireland. An old occluded depression centered over north-west Germany was filling up and moving slowly south-east. Wind speeds of 178 kt (Aldergrove 03 GMT at the 300 mb level) had been reported earlier in the jet stream; the estimated line of strongest winds at 15 GMT extended from just east of Lewis to Glasgow, Chester, Portland Bill and Jersey; the highest reported winds at 300 mb at that hour were 143 kt at Liverpool and 139 kt at Camborne, though 145 kt was observed at 21 GMT at Larkhill.

It was wished to obtain a profile of the wind speeds at short intervals of distance at right angles to the jet stream. Accordingly arrangements were made for a R.A.F. aircraft carrying a vertical camera to fly at 300 mb from Yarmouth on a westerly course to west Wales taking vertical photographs every 12 sec. It was hoped by this means to obtain fixes at short intervals from which to deduce the wind drift. The aircraft was to return via Land's End to the southern Midlands, again taking photographs.

It is believed that this is the first occasion on which the wind profile of a jet stream has been measured at such short intervals of distance. The importance of maintaining a steady height, speed and course was indicated to the navigator, who was asked to keep an accurate position log, and to obtain Gee-fixes* as often as possible.

Photography started near Lowestoft and continued without break on a course of 292° (magnetic) to just beyond 5°W., south of Pembrokeshire. No ground detail was visible in the east, where there was extensive and well-developed cumulus cloud, but from about the Greenwich Meridian to the point of leaving the Welsh coast at the Gower Peninsula the ground was clearly discernible and indentifiable in all save a handful of photographs. From that point to the end of the first leg the cloud over the sea was well broken. The time taken for these 282 photographs was from approximately 1440 to 1535 GMT. Photography recommenced near Helston at 1550 GMT with the aircraft on a course of 041° (magnetic) and a further 158 photographs were taken over south-west England, the run ceasing near Taunton at 1620 GMT; ground detail was unfortunately far poorer on this leg, and in many prints no detail at all could be picked out.

It was not possible to use the photographs to derive winds for the prints east of Greenwich, but the navigator obtained accurate Gee-fixes to this point, and there is no reason to doubt the winds he obtained; accordingly, his winds have been accepted to this point. From 0-4°W. (prints 5070-5236) it was possible in nearly all cases to identify the centre point of the print (i.e. the point at right angles to the plane of the aircraft) and winds were in the first instance calculated between each successive pair of prints (just over a mile). The resulting winds proved very erratic, as might be expected, in view of the effect of the slightest change in altitude of the aircraft magnified over 30,000 ft. Accordingly, five-print means were taken. These gave much more consistent results, but finally means were taken for five-print intervals for which the end points had been adjusted slightly as necessary if they appeared out of harmony with the neighbouring prints; very reasonable winds indeed emerged. There is a short break between 5140-5150 (at approximately 2°W.) due to the obvious divergence of the aircraft momentarily from its track, and the run between prints 5144-5147 has been ignored. The navigator's Gee-winds (meaned over much broader intervals than derived

* Gee-fixes are positions obtained by Gee, i.e. radio transmissions from a master and two slave stations on the ground, the probable error of such positions being about ¼ mi in the best circumstances; at 300 miles distant it may possibly be 5 mi.

from the photographs) agreed very well as far as 2½°W. but at this point the navigator changed Gee-chains; he reported that he had difficulty with the western chain, and could not get really reliable fixes, so in consequence the winds to the west derived from Gee could not be depended upon. It was therefore decided to explore the possibility of assessing the rate of drift across the cloud (mainly thin and well-broken stratocumulus) in the west, and deriving winds. Accordingly winds from rate of drift were obtained near and just east of the Gower Peninsula, and compared with winds derived from ground detail; it was found that if the cloud were given a speed of 56 kt agreement was very close. Accordingly the photographs from 5236-5282 (from 4°W.) were analysed in this manner, mean rate of drift of cloud over five prints being obtained. A very reasonable answer was obtained apart from 5281 and 5282 which appeared to be on a turn.

The winds on the return run were obtained from ground fixes, again utilizing every fifth print. There were some long gaps with no recognizable ground detail (notably 5357-5390, over Dartmoor) and no attempt was made to smooth out the points. The interest of this run was only secondary, as the wind speed increased only slightly as the axis of the jet stream approached.

The winds which were derived on the westerly flight have been plotted on Fig. 1; for convenience the winds have been plotted against the print numbers, longitude and distance from starting point of the run. Full lines correspond with wind derived from photographic analysis, broken lines from Gee-fixes and pecked lines from prints 240-282 show the winds derived from rate of drift across cloud. It will be seen that to almost exactly the point where ground detail became plain, the wind is of the order of 80 kt, and has not been subject to

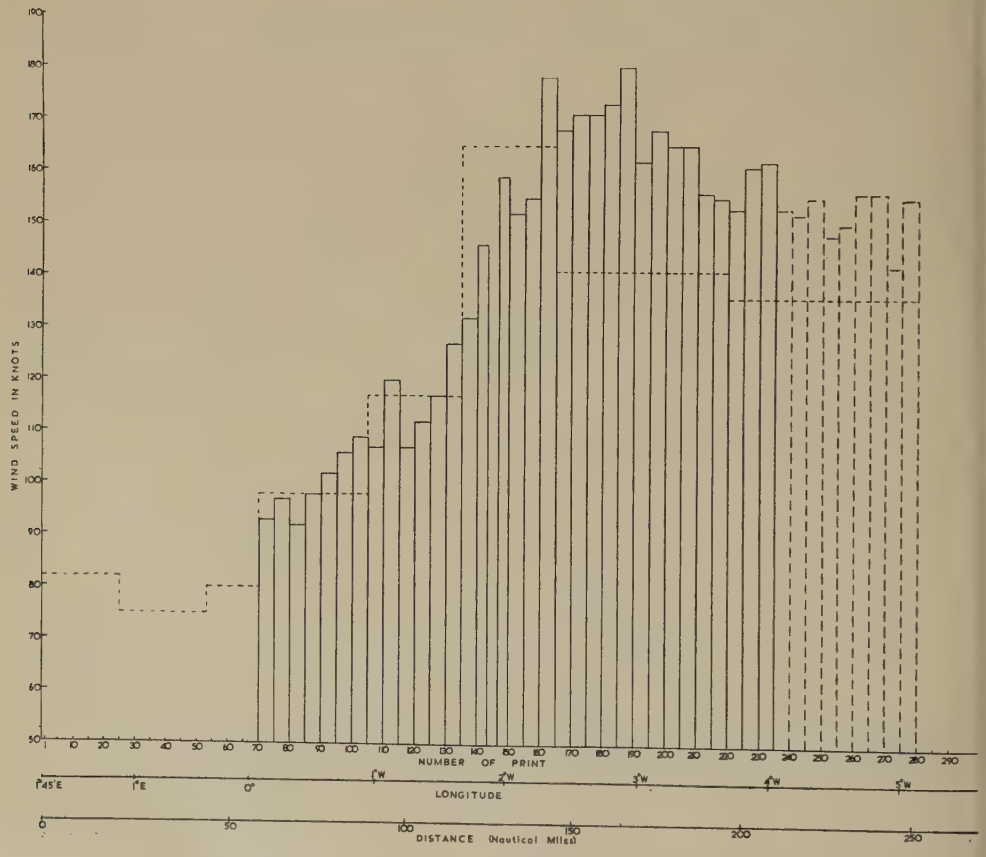

Figure 1. Variation of wind strength with longitude and distance at 300 mb on 18 Jan. 1952 at approximately 15 GMT (see text for explanation of lines).

appreciable change. From this point (60 n mi after the start of the run) there is a fairly steady increase of wind which at the 120-n mi point has reached 130 kt (a gradient of 50 kt/60 n mi). Between this point and the 140-n mi point there is a further increase to 170 kt, (a gradient of 40 kt/20 n mi). A maximum speed of just over 170 kt is maintained for about 20-n mi, and this is followed by a slow fall to the 250-n mi mark where it is 150 kt or perhaps less, as this figure depends on there being no reduction in the speed of the wind at cloud height (5,000 ft-6,000 ft) to the west of the Gower coast. These winds relate well to those actually reported on that day; the maximum wind reported was 178 kt (Aldergrove 300 mb at 03 GMT) compared with a derived 180 kt. The Camborne winds were 156 kt and 139 kt at 09 GMT and 15 GMT respectively, corresponding to positions near, and about 100 n mi to the west of the maximum wind line. The jet stream as a whole was moving east at about 15 kt, so that on the derived figures the increase from 80 kt to 130 kt would take approximately 4 hr at a particular place, and the increase to 170 kt a further 1 to 2 hr. This high speed would only be maintained for an hour or so, after which a slow decrease could be expected. It will be noticed that the Gee-winds shew very good agreement for the first five fixes, but agreement is poor in the west. It is felt that the westerly Gee-winds are in error, and this is supported by the difficulties the navigator experienced with the chain in the west, and by the fact that there was faulty identification of ground detail in the vicinity of Fairwood Common near Swansea. In view of the poorer detail in the return run, its much shorter nature and the absence of any striking change, a similar plot has not been made for this flight. It was however found that a slight increase of wind occurred during this leg of the flight (of the order of 12 kt-15 kt) which corresponds very closely with the change of wind derived from rate of drift across cloud in the outward leg.

Acknowledgments

The original suggestion (for runs in the Middle East) of tackling the problem in this manner was made by Mr. J. S. Sawyer, and the sympathetic assistance of Mr. C. S. Durst in this and allied problems has been much appreciated. Thanks are due to Flt. Lt. R. T. S. Vigers and Fg. Off. F. N. Slaymaker who undertook the flight and to Mr. J. R. Wearmouth who exercised great patience in identifying ground detail in some very difficult photographs. The paper is published by permission of the Director of the Meteorological Office.

551.515.8

A record of the micro-structure of a cold front

By D. R. SAMUEL

Section of Meteorological Physics, Commonwealth Scientific and Industrial Research Organization, Melbourne, Australia

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SUMMARY

A record is presented showing the fine structure of wind speed, azimuth and inclination, temperature and water-vapour pressure taken over a five-minute period within which a cold frontal passage occurred. A particular advantage of the method of recording, using rapid-response instruments, is the graphic illustration provided of the extreme sharpness of the front. The transition zone, estimated to be 70 m in horizontal extent, passed in 8 sec, during which time the temperature fell 5°C and the water-vapour pressure increased by 2 mb.

The record, together with two others taken shortly before and after, has been analysed and the results including the mean vertical velocities, the vertical eddy fluxes of heat, water vapour and momentum, and the K values for heat are presented. Direct evidence is submitted of the vertical eddy transfer of heat, water vapour and momentum through the transition zone.

1. INTRODUCTION

The record was taken during an intensive series of measurements of the vertical eddy fluxes of heat, water vapour and momentum, made at various heights and under different synoptic conditions as part of a programme of investigation into the modification of air masses. One particular situation in which rapid modification might be expected is that in which a cold maritime air mass passes over a warm land surface.

2. EXPERIMENTAL DETAILS

The observations were made at Edithvale, 17 mi south-east of Melbourne, on a flat grassland site, 2 mi inland from Port Phillip Bay. A steel scaffolding tower, 100 ft high, served as a mounting for the instruments. Wind speed and temperature profiles were measured by cup anemometers and aspirated platinum-resistance thermometers at ½, 2, 8, 16 and 32 m levels.

The apparatus used for recording the fine structure of wind speed and inclination, temperature and water-vapour pressure was substantially that described by Swinbank (1951). In order to measure the momentum flux, a simple device has been added which records the instantaneous wind azimuth. The measurements of the fine structure described herein were made at the 30 m level.

3. WEATHER SITUATION

The synoptic chart for SE. Australia for 15 Eastern Standard Time on 16 Jan. 1952 is shown in Fig. 1. It depicts a rather typical situation for SE. Australia at this time of the year, when cold fronts often cross the mainland giving little or no rain and even, on occasion, with little or no cloud over extensive areas. This was the case at Edithvale on the day in question when the sky was cloudless during, and for some hours before and after the frontal passage, which occurred at 1558 EST. But elsewhere in Victoria the front was followed by isolated light showers, and in Tasmania more extensive rain was reported.

A steady sea breeze was maintained at Edithvale for 4 hr prior to the passage of the front.

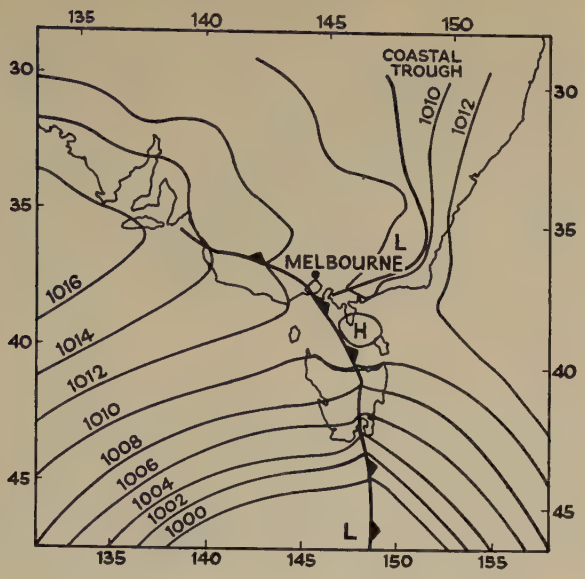

Figure 1. The synoptic chart for SE. Australia for 15 EST on 16 Jan. 1952 (by courtesy of the Commonwealth Meteorological Bureau).

4. The analysis of the traces

The calculation of sensible heat and water-vapour flux, while remaining the same in principle as that proposed by Swinbank (1951), is now carried out on a mechanical analyser specially designed for the purpose by R. J. Taylor. The calculation of the momentum flux is identical with that for heat flux with u, the horizontal component of the wind velocity in the direction of the mean wind, substituted for $C_p T$.

The trace for the five-minute period within which the frontal passage occurred (reproduced in Fig. 2) has been analysed in two sections, (*a*) before the passage of the front and (*b*) after. The transition zone has been excluded from the analysis. The results of the analysis of this trace and of two other traces, one taken before and the other after, appear in Table I.

TABLE I. Results of the analysis of 3 records taken at a height of 30 m at Edithvale on 16 Jan. 1952

Run 2 contains the front, section (*a*) being before the passage and section (*b*) after. The sign convention is positive upwards

Run	Time (EST)	$\bar{u}$ (m sec^{-1})	$\overline{w}$ (cm sec^{-1})	Heat flux (erg cm^{-2} sec^{-1})	Water-vapour flux (g cm^{-2} sec^{-1})	Momentum flux (dyne cm^{-2})	$\frac{\partial \overline{T}}{\partial z} + \Gamma$	K heat (cm^2 sec^{-1})
1	1536-1541	2·6	+ 17	$28{\cdot}5 \times 10^4$	$5{\cdot}5 \times 10^{-4}$	− 0·01	$-2{\cdot}5 \times 10^{-4}$	$8{\cdot}5 \times 10^4$
(*a*)		5·2	+ 17	$7{\cdot}4 \times 10^4$	$3{\cdot}5 \times 10^{-4}$	+ 1·5	$-1{\cdot}5 \times 10^{-4}$	$3{\cdot}9 \times 10^4$
2	1556-1601							
(*b*)		12·9	− 19	$13{\cdot}5 \times 10^4$	$11{\cdot}0 \times 10^{-4}$	− 3·7	$-2{\cdot}0 \times 10^{-4}$	$5{\cdot}2 \times 10^4$
3	1614-1619	10·9	+ 2·5	$7{\cdot}8 \times 10^4$	$7{\cdot}2 \times 10^{-4}$	− 2·4	$-3{\cdot}0 \times 10^{-4}$	$2{\cdot}0 \times 10^4$

The eddy-transfer coefficients for heat are also listed in Table I. The transfer coefficients for water vapour and momentum are not given because on this occasion the humidity gradients were not measured and the differences in wind speed between 16 m and 32 m were too small to measure with sufficient accuracy.

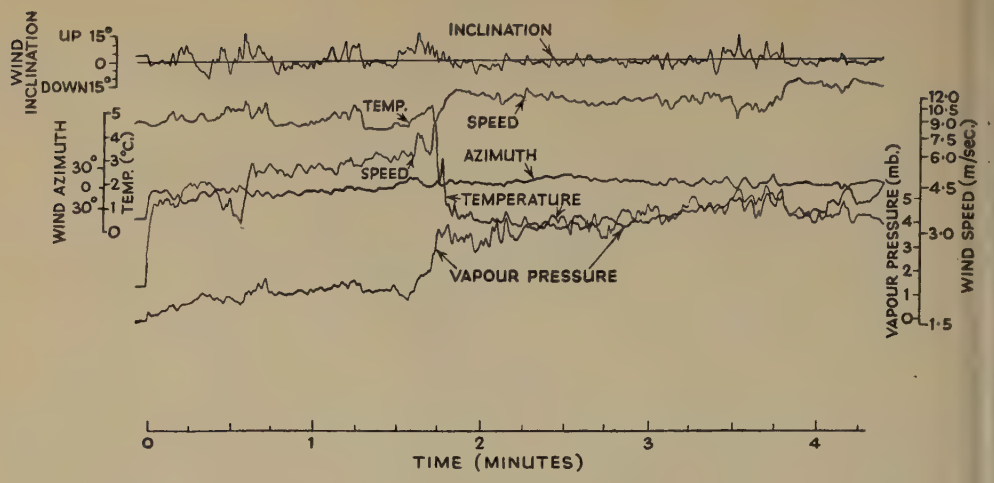

Figure 2. Record of the fine structure of wind speed, azimuth and inclination, temperature and water-vapour pressure at 30 m for the five-minute period within which the frontal passage occurred. Temperature and water-vapour pressures are measured from arbitrary zeros.

5. Discussion of the results

(*a*) *Vertical velocities*

A knowledge of the vertical velocities which occur in the region of a frontal surface is fundamental to any discussion of the structure of fronts and a number of methods of estimating the sign and magnitude of these velocities has been proposed. The foregoing records are believed to be the first direct measurements of vertical velocities in the region of a front, though referring, of course, only to that part near to the ground. The mean vertical velocity, $\bar{w}$, changed from 17 cm sec^{-1} upward during section (*a*) to 19 cm sec^{-1} downward during section (*b*).

Although the error in these measurements of $\bar{w}$ may be as large as 10 per cent the measured change in $\bar{w}$ from section (*a*) to section (*b*) of 36 cm sec^{-1} would be accurate to within a few per cent.

(*b*) *Cross section at 30 m*

An estimate of the velocity and inclination of the frontal surface at 30 m can be made from kinematic considerations. The kinematic boundary condition is that $\bar{w} = (C - \bar{u}_n) \tan \theta$ where $\bar{u}_n$ is the horizontal component of the wind velocity normal to the front, C is the velocity of the front and θ the inclination of the frontal surface. Synoptic evidence indicates that locally the mean wind direction was normal to the front so that $\bar{u}_n$ is equal to $\bar{u}$. Substitution of the measured values of $\bar{w}$ and $\bar{u}$ for the warm and cold air gives a slope of 1 in 20 and a front velocity of 9 m sec^{-1}.

The well-defined zone of transition occupies 8 sec of the trace and, from the estimated front velocity, was approximately 70 m in horizontal extent. For a slope of 1 in 20 this means the transition zone was roughly 3 m thick.

(*c*) *Wind shift*

Inspection of the azimuth trace in the record reproduced in Fig. 2 shows that, at Edithvale, practically no wind shift occurred with the passage of the front. That this is a local effect, probably caused by the presence of the sea breeze, can be seen by considering the wind shifts that occurred at several nearby stations. The positions of these stations are shown in Fig. 3. A sea breeze was recorded at Laverton, Central Weather Bureau and Edithvale some hours before the front.

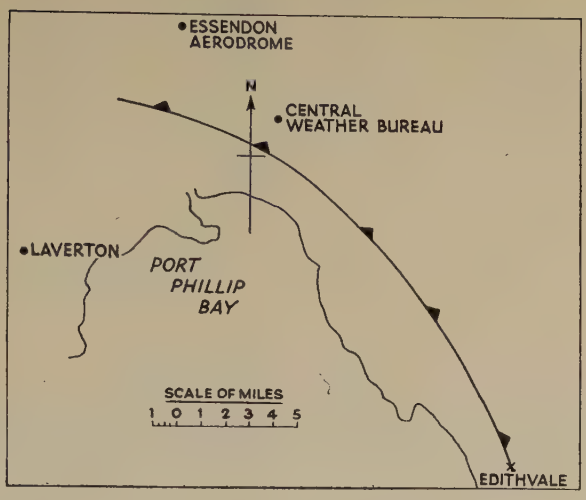

Figure 3. The location of observing stations and the estimated position of the front at 1558 EST.

At Edithvale and Central Weather Bureau there was practically no wind shift with the passage of the front whereas at Laverton, situated on the opposite side of Port Phillip Bay to Edithvale, a clockwise shift from east to south occurred. The sea breeze did not reach Essendon and here the normal anticlockwise shift from west to south occurred.

(*d*) *Eddy flux across the transition zone*

An unusual feature of the eddy fluxes in Table I is the upward transfer of momentum during run 2 (*a*) and the implied negative vertical gradient in $\bar{u}$ (i.e., $\bar{u}$ decreasing upwards). This is to some extent substantiated by the anemometer readings. The mean gradient at 30 m over the full five-minute period was almost zero and hence the downward momentum flux in the latter part implies both a positive velocity gradient during that part, and a negative gradient during the first part.

This could be explained by the transfer of momentum from the underlying cold air into the slower-moving warm air. A transfer of heat and water vapour across the boundary should also occur. Fig. 4 indicates the direction in which the transfer of heat, water vapour and momentum would be expected from the relative properties of the two air masses. From this it would appear that there might be some region, close to the boundary, in which the eddy flux of water vapour is increased and the fluxes of heat and momentum are reversed, or at least reduced.

Figure 4. The directions in which the transfer of heat, water vapour and momentum would be expected from the relative properties of the two air masses.

To test this hypothesis run 2 (*b*), taken immediately after the passage of the transition zone, was analysed in three equal subdivisions, the results appearing in Table II. The agreement of these results with the hypothesis is reasonably good, which in itself provides some reassurance concerning the significance of fluxes measured over intervals as short as this.

TABLE II. THE EDDY FLUXES OF HEAT, WATER VAPOUR AND MOMENTUM MEASURED OVER THREE EQUAL SUBDIVISIONS OF RUN 2 (*b*)

	Heat flux erg cm^{-2} sec^{-1}	Water-vapour flux g cm^{-2} sec^{-1}	Momentum flux dyne cm^{-2}
(i)	$- 1{\cdot}5 \times 10^4$	$9{\cdot}9 \times 10^{-5}$	$- 1{\cdot}4$
(ii)	$+ 23{\cdot}5 \times 10^4$	$7{\cdot}7 \times 10^{-4}$	$- 2{\cdot}6$
(iii)	$+ 4{\cdot}9 \times 10^4$	$7{\cdot}5 \times 10^{-4}$	$- 3{\cdot}5$

6. FUTURE INVESTIGATIONS

The frontal passage discussed here is, synoptically, typical of many cold fronts which occur in this region at this time of year. Whether the sharp discontinuity revealed by the fine-structure record presented above is an unusual feature or is typical of such fronts is as yet unknown and must await further work. During the course of the general investigation of the eddy fluxes of heat, water vapour and momentum further attempts will be made to secure records of the micro-structure in the region of discontinuities. Two other such examples have so far been obtained, one showing the passage of another cold front, though less sharply defined, and the other showing the onset of a sea breeze.

ACKNOWLEDGMENTS

This study was undertaken during a wider investigation shared by members of the Section of Meteorological Physics, C.S.I.R.O. Thanks are due to them and in particular to Mr. W. C. Swinbank and Mr. I. McIlroy for their helpful suggestions in the preparation of this account. Thanks are also due to Mr. J. N. McRae of the Commonwealth Meteorological Bureau for his analysis of the synoptic material.

REFERENCE

Swinbank, W. C. 1951 *J. Met.*, **8**, p. 135.

551.513.1 : 551.515.8

A note on an intertropical discontinuity

By J. R. SCOTT

Malayan Meteorological Service, Singapore

(Manuscript received 20 June 1952, in revised form 28 July 1952)

Summary

The existence of a line or zone in which air flow has zero vorticity is inferred from the equation of motion. This zone is called the Intertropical Discontinuity. The detection, development and movement of this zone are discussed. It is shown how knowledge of its location and properties might be of value to forecasting near the equator.

1. Definition

In this note the unqualified word vorticity means the absolute vorticity about the vertical, i.e., the sum of the Coriolis parameter and the relative vorticity about the vertical. Also, the word divergence is used in its mathematical sense and includes convergence as negative divergence. Where this is not true is indicated by the context.

When terms inversely proportional to the radius of the earth and those containing the vertical velocity are neglected, and when there is little horizontal variation of density, it may be shown that the equation of motion of air reduces to

$$\frac{\partial \mathbf{V}}{\partial t} + \frac{1}{\rho} \nabla (p + \tfrac{1}{2}\rho V^2) = -(\lambda + \zeta)(\mathbf{k} \times \mathbf{V}) + K \frac{\partial^2 \mathbf{V}}{\partial z^2} \quad . \quad . \quad (1)$$

in which $\mathbf{V}$ is the horizontal velocity, ∇ indicates a horizontal gradient, ρ is density, p the pressure, λ the Coriolis parameter, ζ the relative vorticity about the vertical, $\mathbf{k}$ the unit vertical vector, K the coefficient of eddy viscosity and z increases with height.

This equation approaches complete rigour when applied to a thin layer of air in contact with the surface of the earth. With the omission of certain terms it reduces to the well-known geostrophic relation which indicates that vorticity of low-level flow in temperate latitudes always has the same sign as the Coriolis parameter; i.e., it is always positive in the temperate latitudes of the northern hemisphere and always negative in those of the southern hemisphere. It follows that there must exist between them an odd number of zones across which vorticity changes sign.

In the trade winds of either hemisphere, gradients of $\frac{1}{2}\rho V^2$ are small compared with pressure gradients and low-level flow is comparatively steady and predominantly along the surface isobars. The small component of flow across the isobars towards low pressure may be legitimately ascribed to friction. Hence, in these streams it may be inferred, from Eq. (1), that the vorticity is finite, and its sign can be found from the sense of flow along the isobars. In the trades of the northern hemisphere vorticity is positive and in those of the southern hemisphere it is negative. Proceeding polewards from the trades the numerical value of the Coriolis parameter increases and the geostrophic relation becomes progressively a more useful approximation. This demonstrates that nowhere between the trades and the polar regions does vorticity change sign. It is, therefore, reasonable to suppose and experience suggests that, in general, there is only one zone across which vorticity changes sign, and that it lies between the trade winds of the two hemispheres. This zone will be called the Intertropical Discontinuity (I.T.D.). Its width can presumably vary from almost zero to several hundreds of miles. To cater for this variation and to make the definition a practical one the zone will be defined as one in which a finite vorticity cannot be detected and which, in the limit, can appear as a discontinuity across which vorticity changes sign abruptly.

2. Detection

The following rules can be inferred from Eq. (1) :

(*a*) A steady finite wind blowing approximately along the isopleths of $p + \frac{1}{2}\rho V^2$ does not lie in the I.T.D. If its direction of flow has the same sense as that of movement along isobars in the northern hemisphere the I.T.D. lies to the south of the observation, and vice versa.

(*b*) Where the gradient of $p + \frac{1}{2}\rho V^2$ and friction are negligible the I.T.D. lies to the south of a finite wind which has veered, and to the north of a finite wind which has backed. A finite wind which has not changed lies within the I.T.D.

(*c*) If there is any gradient within the I.T.D. the wind will tend to blow down it.

Eq. (1) cannot be used to make inferences about areas of light and variable winds. However in such an area it may be assumed that relative vorticity is small, and if the area lies within two or three degrees of the equator the vorticity is small. Hence, it may be assumed that doldrum areas near to the equator always lie in the I.T.D.

Just outside the I.T.D., particularly when its edges are moving, it is likely that all the terms of Eq. (1) are finite. In these circumstances it will be difficult to detect finite vorticity. A possible working rule would be to assume that the change of a finite wind to one more along the isopleths indicates that the I.T.D. has moved so that it no longer covers the observation, and vice versa.

Previous analyses should help in location because the edges of the I.T.D. cannot generally move faster than the winds in their neighbourhood. The justification for this statement and exceptions to the rule are discussed in Section 4.

A further help in analysis is the fact that the I.T.D. does girdle the earth and therefore the joining of detectable points on it is a legitimate process.

3. Properties

(*a*) The I.T.D. is neither a front nor necessarily a zone of convergence. It merely has the property that vorticity is zero in it.

(*b*) If it is narrow it ought to be possible to make useful deductions about the existing field of low-level divergence since the air outside it has a tendency to flow along the isopleths. This tendency permits the inference of convergence to the west and divergence to the east of lows of $p + \frac{1}{2}\rho V^2$ through which the I.T.D. passes.

(*c*) It cannot become involved in either tropical cyclones or the summer depressions of continents situated in the tropics because the circulation round these always shows vorticity of sign appropriate to the hemisphere in which they exist. This suggests that it never gets far from the equator, though it probably achieves its greatest width and diffuseness when the trade winds are replaced by these disturbances.

(*d*) If the well known equation,

$$\frac{d}{dt}(\lambda + \zeta) = -(\lambda + \zeta) D \quad . \quad . \quad . \quad . \quad (2)$$

(in which D is horizontal divergence) is true, the I.T.D. forms a dynamical barrier which prevents the passage of air through it. This, in conjunction with (*c*) above suggests that large meridional movements of air do not occur across the equator at low levels : a conclusion which is not well supported by mean wind observations. In particular, the apparent movement of the south-east trades of the western Indian Ocean into the south-west monsoon of the Arabian Sea, which wind stream affects the greater part of India, suggests that there must be a mechanism for vorticity to change sign. Such a mechanism is discussed in Section 4.

4. Development and movement

Eq. (2) shows that convergence in air near to the I.T.D. will make it more distinct, while divergence has the opposite effect. In fact, a paradox is presented by this equation because it suggests that air may ' pass into ' the I.T.D. by prolonged divergence but it does

not suggest any mechanism by which air can leave it. If air can enter the I.T.D. there must be a method of exit, and this must be provided by terms which were neglected in the derivation of Eq. (2). Apart, possibly, from the extremely strong vertical currents in thunderstorms it is possible to make terms involving vertical velocity as small as we please by reducing the thickness of the layer in contact with the surface to which the equation is to be applied, and we may then expect friction, which has been neglected in the derivation of Eq. (2), to be an important factor. Friction is accounted for by the addition of $K\partial^2\zeta/dz^2$ to the right-hand side of the equation and since the Coriolis parameter does not vary with height this term may be written $K\partial^2(\lambda + \zeta)/\partial z^2$. Thus under suitable variation of vorticity with height vorticity of the same sign as that of the overlying air or the underlying earth may be frictionally induced, while surface friction can alone induce vorticity in air not actually at the equator. These effects, then, provide one mechanism for a change in the sign of vorticity and, therefore, for the passage of air through the I.T.D., and it may be inferred that the I.T.D. will tend to move with a current of air aloft which can be traced back to a source away from the equator.

Frictional effects are, however, absent when the change of vorticity with height is linear, and it is towards this state that friction works. It is pertinent, therefore, to consider how the I.T.D. can move when friction is absent.

Within the I.T.D. in the absence of friction

$$\frac{d}{dt}(\lambda + \zeta) = \lambda + \zeta = 0$$

Therefore

$$\frac{\partial}{\partial t}(\lambda + \zeta) = -\mathbf{V} \cdot \nabla(\lambda + \zeta) \quad . \quad . \quad . \quad . \quad (3)$$

Eq. (3) shows that the southern edge of the I.T.D. can only move northwards and the northern edge southwards by the advection of vorticity. If the edges are close and $\partial V/\partial t$ small on both sides, the ' rate of arrival ' of vorticity at the I.T.D. from one side has the same magnitude though of opposite sign from that from the other, for isopleths of $p + \frac{1}{2}\rho V^2$ are continuous and the gradient at the narrow I.T.D. is common to both sets of arriving air. It may be concluded that a narrow I.T.D. tends to remain stationary, or if movement occurs it must take place in two stages. The first is a broadening either by prolonged divergence in the air near it or by advection of air from another part of it. The second stage is the advection of vorticity. It may be inferred that in conditions where friction is negligible the development of a broad I.T.D. must precede movement.

Convergence in the lowest levels is often associated with bad weather because of the high water-vapour content there. The rate of change of divergence in the I.T.D. at low levels should, therefore, give some indication of weather trends in it. By operating on the scalar equations which were added vectorially to produce Eq. (1) it may be shown that

$$\frac{\partial D}{\partial t} = -\frac{1}{\rho}\nabla^2(p + \tfrac{1}{2}\rho V^2) - (\mathbf{k} \times \mathbf{V}) \cdot \nabla(\lambda + \zeta) + K\frac{\partial^2 D}{\partial z^2} \quad . \quad . \quad (4)$$

holds in the I.T.D., ∇^2 being the Laplacian applied to the horizontal plane only. Eq. (4) demonstrates the importance of isopleths of $p + \frac{1}{2}\rho V^2$, and suggests that bad weather will develop near the edge of the I.T.D. when the wind there has a component parallel to it in the direction of travel of a point circumscribing the globe eastwards along the I.T.D. edge.

5. Conclusion

This note is an attempt to define and discuss briefly that entity which the writer thinks most equatorial meteorologists have in mind when they talk about the intertropical front. The I.T.D. is, in the absence of frictional effects, the line or zone separating air of northern and southern hemisphere origin, and this is a property often claimed for the intertropical front.

The writer has used the rules of Section 2 and, since gradients of $\frac{1}{2}\rho V^2$ are usually negligible, the surface-pressure field to locate the I.T.D. Some improvement in consistency and confidence of analysis has been achieved. Should more ' representative ' observations

of wind and pressure become available it may be possible to predict, on the lines indicated in this note, the movement of and the development of weather within the I.T.D. This would not, however, provide means of forecasting the development and movement of the many areas of bad weather which appear in the equatorial regions and which cannot be reasonably associated with the I.T.D.

Appendix

Apart from the neglect of vertical velocity, referred to in Section 1, the only questionable assumption made in deriving Eq. (1) is that terms involving the horizontal variation of density could be neglected. It is the purpose of this appendix to show that the neglect of such terms is permissible in the equatorial regions.

To take account of horizontal density variation the term $V^2\nabla\rho/2\rho$ must be added to the right-hand side of Eq. (1). Horizontal temperature gradients are very small, and the assumption that horizontal density gradients are entirely due to pressure gradients cannot introduce serious error. This assumption can readily be shown to require a wind speed of about 1,000 kt for the above term to equal the pressure term needed to produce it. A wind speed of as much as 200 kt produces a value of the term of only 0·05 of the magnitude of the associated pressure term.

With regard to the effect of horizontal density variation on Eq. (2) the writer (Scott 1951) has shown that if ∇p is parallel to $\nabla\rho$ the term in Eq. (2) containing $\nabla\rho$ is identically zero, and that this condition usually exists in the equatorial regions. In the same publication the neglect in equatorial regions of the term containing $\nabla\rho$ which would appear in Eq. (4) is also justified.

Reference

Scott, J. R. 1951 *Mem. Malayan Met. Service*, No. 5.

551.510.41

The effect of secondary scattering in the calculation of the vertical distribution of atmospheric ozone from the Götz inversion-effect

By K. R. RAMANATHAN, Bh. V. R. MOORTHY and R. N. KULKARNI
Physical Research Laboratory, Ahmedabad, India

(Manuscript received 20 June 1952, in revised form 29 July 1952)

To determine the vertical distribution of ozone in the atmosphere, two methods are in general use : (*a*) measurement of the intensities of lines in the solar spectrum in the Hartley band from photographs taken at different heights in the atmosphere with spectrographs sent up with sounding balloons (E. and V. H. Regener 1934, E. Regener 1952), or in V_2 rockets (U.S. Nav. Res. Lab. 1947, Götz 1951) and (*b*) by analysing the ratio (I/I') of the intensities of the light coming from the zenith sky at different zenith distances of the sun in two narrow spectral bands, say λ 3112 and λ 3323, the first of which is in the ozone-absorption region and the other just outside that region (Götz *et al.* 1934). The phenomenon discovered by Götz that the ratio I/I' decreases with increasing zenith distance of the sun up to about 85° and increases thereafter is known as the ' umkehr ' or inversion-effect.

The calculation of the ozone distribution from the inversion-effect assumes that $I/I' = P/P'$ where P and P' are the intensities of the light *primarily* scattered downwards along the vertical by the molecules of the air, making due allowance for the attenuation of the light by molecular scattering and ozone absorption. The neglect of higher orders of scattering in the calculation of I/I' is a source of error in the determination of vertical distribution.

The error involved would be expected to be small if the scattering and absorption coefficients for λ 3112 and λ 3323 were nearly the same; for then, multiple scattering will affect I and I' by nearly the same factor and we are concerned with I/I'. Actually the absorption coefficient of ozone for λ 3112 is much larger than that for λ 3323 and as a consequence there will be, at low altitudes of the sun, comparatively much more multiply-scattered light of λ 3323 from the denser, lower part of the atmosphere than of λ 3112. The observed ratio I_{3112}/I_{3323} will be smaller than the calculated ratio P_{3112}/P_{3323}.

It should be possible to calculate the effect of the multiple scattering in the zenith sky light by the methods introduced by Chandrasekhar (1950), but that has not yet been done. In the meantime, actual measurements of the umkehr effect made on the same morning or evening with different pairs of wavelengths enable us to obtain some idea of the errors involved in neglecting multiple scattering.

A Dobson ozone spectrophotometer of the modified type in which the shorter wavelength can be varied from about 3040 A to 3200 A and the longer from 3240 A to 3430 A has been in regular use at Mount Abu (24° N.) since October 1951. With the cloudless weather which prevails at this station for nearly nine months in the year, it has been possible to obtain umkehr-curves from the zenith sky on the same day for the pairs of wavelengths 3112-3323 and 3075-3278 on a number of days. The vertical distributions of ozone have been calculated from observations on each of these pairs of wavelengths and also with the pair 3112-3075 by eliminating the effects of 3323 and 3278. When the wavelengths under comparison are 3112 and 3075, both of which are strongly absorbed by ozone, the effects of multiple scattering on the two wavelengths roughly balance out each other. Similarly when the little-absorbed wavelengths 3323 and 3278 are compared, there is again an approximate balance.

The vertical distribution obtained from observations on wavelengths 3112-3323 is nearly the same as that obtained from those on 3075-3278 but the distribution obtained with 3075-3112 is markedly different, the centre of gravity of ozone in the last case being lower.

Table I gives comparative figures of distribution for 3 days with ozone amounts 0·153 cm, 0·188 cm and 0·209 cm respectively.

TABLE I. Vertical distribution of ozone from Götz inversion-effect at Mount Abu (24° 40′ N India.

Date	$10^3 \times$ amount of O_3 in cm	Pairs of wave lengths used	Amount of O_3 in different layers (Each layer is 9 km thick. Layer 1 is 0-9 km, layer 2, 9 km-18 km, etc.)				
			1	2	3	4	5
21 Oct. 1951	153	(a) 3075/3278	9	9	62	59	9
		(b) 3112/3323	0	9	62	68	12
		(c) 3075/3112	9	18	78	41	5
		(c) − (b)	9	9	16	− 27	− 7
5 Feb. 1952	188	(a) 3075/3278	0	16	97	54	13
		(b) 3112/3323	0	18	99	53	14
		(c) 3075/3112	9	27	103	40	5
		(c) − (b)	9	9	4	− 13	− 9
7 Feb. 1952	209	(a) 3075/3278	5	13	111	63	13
		(b) 3112/3323	9	13	111	64	11
		(c) 3075/3112	0	18	131	36	15
		(c) − (b)	− 9	5	20	− 28	4

In all three instances, the effect of the substitution of λλ 3075-3112 for 3112-3323 on 3075-3278 is to increase the ozone content in the layers 9 km to 27 km and decrease it in the layer 27 km to 36 km. The maximum day-to-day variations occur in the layer 18 km to 27 km. The lowering of the centre of gravity of the ozone as a result of these revised calculations enhances the significance of ozone measurements for elucidating movements in the upper troposphere and lower stratosphere even in sub-tropical latitudes.

These results and also the day-to-day vertical ozone distributions will be discussed more fully elsewhere.

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551.521.14

Note on the reflection of diffuse daylight from still water

By W. E. KNOWLES MIDDLETON
National Research Council, Ottawa, Canada

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SUMMARY

The luminous reflectance of still water for the light from an overcast sky is found to be less than the commonly accepted value ($R = 0{\cdot}066$), because such skies are brighter at the zenith than at the horizon. For a sky three times as bright at the zenith as at the horizon, the value $R = 0{\cdot}051$ is computed.

H. Griesseier (1952) has recently given a theory of the diffuse reflecting power of still water on the assumption that the source of light is a sky of uniform luminance in all directions. Making certain approximations for the sake of an analytic solution, Griesseier obtains the value $R = 0{\cdot}056$. Actually the theory of this had been worked out much earlier by McNicholas (1928), and Judd (1942) published computations of the reflectance of diffusely incident light from plane boundaries of various relative indices of refraction m. His value for water in air ($m = 1{\cdot}33$) is $R = 0{\cdot}0660$, and as this was obtained by numerical integration we may assume that Griesseier's value is in error because of the approximations that he made.

The present note is intended to draw attention to the fact that a uniformly luminous sky is not the rule even under a heavy overcast, as was pointed out by Moon and Spencer (1942), who showed that numerous observations of the luminance of overcast skies can be represented fairly well by the formula

$$B_\xi = B_h (1 + 2 \cos \xi) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where B_ξ is the luminance at zenith angle ξ, B_h that of the sky at the horizon ($\xi = \pi/2$). The present writer (1947) developed some of the other consequences of this. Let us now determine its effect on the reflectance of still water, taking as before a rather more general relation

$$B_\xi = B_h (1 + n \cos \xi) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The definition of the total luminous reflectance R of such a plane is in terms of the total amount of luminous energy incident and reflected. The ratio of these is determined by measuring the normal illumination on the upper and lower sides of a small horizontal plane surface $d\sigma$ suspended above the (large) water surface at a distance much greater than the dimensions of the small plane. The proof of the adequacy of this procedure is simple and depends on the equality of the angles of incidence and reflection.

Consider polar co-ordinates ϕ (azimuth) and ξ. Since the sky is assumed axially symmetrical, we need consider only a portion of the sky bounded by planes $\phi = \phi_1$ and $\phi = \phi_1 + d\phi$. An element of sky dS in this will subtend a solid angle $d\omega = \sin \xi \, d\xi \, d\phi$ at the surface $d\sigma$.

Each element of sky will be mirrored in the still water, and an element whose luminance is B_ξ will have a mirror image of luminance $B_\xi R_\xi$, where R_ξ is the Fresnel reflectance of the water at angle ξ. Tables of this are available (Moon 1940). The luminous flux from an element of the actual sky to $d\sigma$ will be

$$dF' = B_\xi \sin \xi \cos \xi \, d\xi \, d\phi \, d\sigma \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and from the corresponding element of the reflected sky

$$dF = B_\xi R_\xi \sin \xi \cos \xi \, d\xi \, d\phi \, d\sigma \quad . \quad . \quad . \quad . \quad (4)$$

The total reflectance is immediately found by integrating Eqs. (3) and (4) from 0 to $\pi/$ and dividing; but we may first cancel out the constant factors B_h, $d\phi$, and $d\sigma$. Therefor

$$R = \frac{\int_0^{\pi/2} (1 + n \cos \xi) R_\xi \cos \xi \sin \xi \, d\xi}{\int_0^{\pi/2} (1 + n \cos \xi) \cos \xi \sin \xi \, d\xi} \quad . \quad . \quad . \quad (5$$

The integrations are performed numerically.

The relation between R and n is as follows :

n =	0·0	0·5	1·0	1·5	2·0	2·5	3·0
R =	0·0659	·0593	·0553	·0527	·0508	·0493	·0482

The value for $n = 0$ agrees with that of Judd (1942) to within the accuracy ascribed by him to his results.

In view of Eq. (1) it would seem that 0·051, and not 0·066, is the appropriate value of F under an overcast sky. Experimental confirmation is difficult because of the uncertain amoun of light scattered back by the water below the surface.

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Records of snowfall in Britain

By S. E. ASHMORE

Borough Meteorologist's Office, Wrexham

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Summary

Different stations do not all record with equal fidelity the number of days on which snow falls. To obtain intercomparability of records, equations are given by which corrections can be applied to records which have missed some of the snowfall.

1. Introduction

Climatological workers may require to produce a map of the average annual number of days with snow over an area for a given period, as has been done for the British Isles by Manley (1940); or snowfall means may be used as a check on the estimates of mean temperature over a period (Manley 1952). In such work it has become evident that the quantity ' number of days with snow ' is of an uncertain order of accuracy, and cannot be extracted from publications and utilized with the same degree of confidence as, say, a rainfall total. Professor Manley has commented in correspondence on the considerable variations in the annual totals of days with snow at pairs of adjacent stations, where one would expect roughly equal totals, for example, Kew and Wisley. It is at his suggestion that the following investigation into these variations is made.

2. Variations in annual totals of snow-days

The definition of a day of snow is ' a day on which sleet or snow fell ' (regardless of the amount), and hence it is a quantity dependent on eye-observation over a continuous period. Also, according to definition, a day on which, for instance, only a few flakes fall out of a stratocumulus cover, and easily missed, is ' a day of snow.' Only at a minority of stations are records compiled from the observations of those whose chief task is to watch for such happenings; but accurate records are known to be obtained by other observers. At a certain station what is thought to be an accurate record is obtained by attention to the following matters : (*a*) careful watch for a few flakes falling in cloudy anticyclonic conditions; (*b*) being on the alert when it appears that slight snow may occur after dark, and going out with a lamp; (*c*) observation of the ground, especially when frozen, for small powderings which may have occurred in the night; (*d*) having more than one trained observer on watch; (*e*) having one or two careful policemen who report on the early morning hours; (*f*) careful examination of precipitation in maritime polar air, which may produce hail, snow, and sleet alternately; (*g*) observation of the windscreen of a car when motoring in rain at a temperature below 40°F, or of a piece of glass which is left ready in an open shed, to acquire the temperature of the air, and any rain suspected of containing sleet is allowed to impinge on it and the glass illuminated during darkness from the other side; (*h*) careful attention to the period around midnight during possibly snowy conditions. A fall of snow from 2355 hr to 0002 hr means two days of snow, which, in the absence of more snow, would both go unrecorded.

Many observers are not in a position to arrange for such vigilance, and their totals will be lower owing to the calls of their occupation, and the impossibility of arranging a continuous watch. Again, observations of snow may be made, and, by inadvertence, not recorded, and slight snow at a health resort may not be recorded owing to reluctance (the writer has had experience of this). The Meteorological Office is aware of lack of uniformity in the standard of observations of snow, and the problem merits quantitative investigation.

3. Quantitative estimates

For comparisons, the record at Wrexham from 1942 to 1950 was used. It was at one time supported by the record of a neighbouring airfield, it is believed to contain a record of every fall of snow, and has detailed descriptions of each fall. The total for each year was put down, the figures ranging from 11 in 1943 to 68 in 1947. This was taken as category A 'first-class observations.' From examination of the records, columns were then made of the number likely to be recorded by observers in categories B, 'good standard,' C 'moderate' and D 'lower grade.' It was subsequently found necessary to include a category A −, 'very good,' which closely approaches A and includes all the falls of practical importance. The total number of intercomparisons of these categories is 10, and this number of graphs was drawn, one of them, relating A and C stations, being reproduced in Fig. 1. On it, each point represents a year's total, and the blackened circle represents two coincident points.

In each case a close fit to a straight line was obtained, the equations being of the form $n_X = an_Y + b$ where n_X is the number of days of snow recorded by a station X of a higher category and n_Y is the number recorded by a station Y in a lower category.

The constant a is related to the number of hours in the day during which watch can be kept, and b is related to the vigilance possible during that period. The actual equations are: $n_A = 1{\cdot}05\, n_{A-} + 1$....(1); $n_A = 1{\cdot}1\, n_B + 3$....(2); $n_A = 1{\cdot}25\, n_C + 5$....(3); $n_A = 1{\cdot}46\, n_D + 6{\cdot}4$....(4); $n_{A-} = 1{\cdot}04\, n_B + 2$....(5); $n_{A-} = 1{\cdot}19\, n_C + 3{\cdot}8$....(6); $n_{A-} = 1{\cdot}39\, n_D + 5$....(7); $n_B = 1{\cdot}2\, n_C + 1{\cdot}6$....(8); $n_B = 1{\cdot}32\, n_D + 3{\cdot}2$....(9); $n_C = 1{\cdot}17\, n_D + 2{\cdot}4$....(10).

4. Classification of stations

From the *Monthly Weather Report* 76 records of days of snow were extracted over as long a period as possible, the periods lying mainly between 1917 and 1950. The records came from 71 stations (a few of the records were found to lack homogeneity and were treated as more than one) and the average annual number of snow-days and of days of snow lying were computed. The stations had been chosen so that they could be cast into groups, each of between two and five stations, their records covering the same period of years; each group

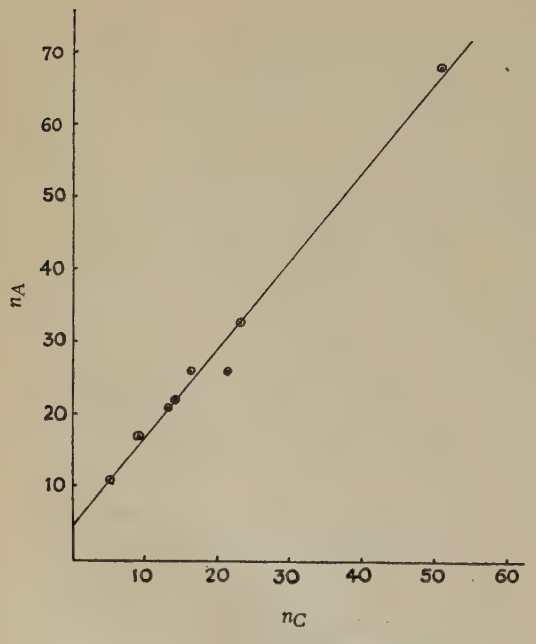

Figure 1

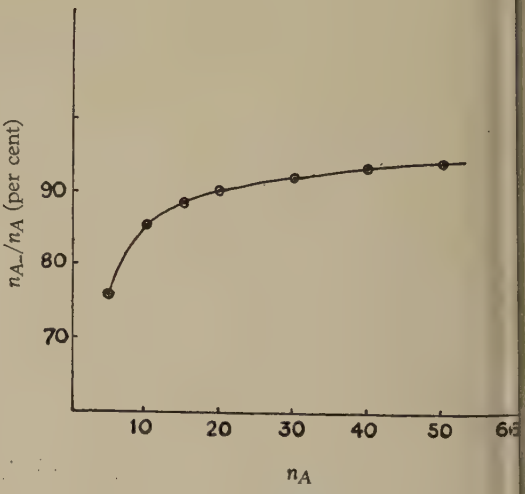

Figure 2

contained stations, (*a*) reasonably close together, (*b*) of similar altitude and geographical position, (*c*) of similar mean temperature, (*d*) with about the same number of days of snow lying, (*e*) containing where possible a station apparently of category A or A −. Within each group the average annual totals of snow days were expressed as percentages of each other. If stations X, Y and Z gave averages in that order, then Y/X, Z/X, Z/Y, were the percentages calculated. Eqs. (1) to (10) were then used, and for each equation the average annual total of snow days at the station of lower category was expressed as a percentage of the average at the station of higher category for average totals of snow-days at the station of higher category ranging from 5 to 60 days. For instance, from Eq. (1), n_{A-}/n_A was expressed as a percentage of n_A for varying values of n_A. These results were plotted on graphs, one for each equation; that for Eq. (1) is given in Fig. 2.

Taking each group of records in turn, the percentages obtained from the means within the group were then compared with the graphs and, from correspondence of percentages, it was found easy to place the stations in their appropriate categories.

5. Results

74 of the 76 results were classified. Since the purpose of this paper is not to extol or criticize the records of any particular station, it is adequate to summarize the results as in Table I, where the stations are given their official classification, I (first order), II (second order), III (third order), T (telegraphic), and a few cases where the order changed without affecting the homogeneity of the records.

TABLE I. Classification of stations

Category	A	A −	B	C	D
I	12	5	2	0	0
II	1	2	4	1	2
III	3	6	13	9	8
T	0	2	1	0	0
T becoming III	0	0	0	2	0
II becoming III	0	0	0	1	0
Total	16	15	20	13	10
Percentage of all records	21·5	20·5	27·0	17·6	13·5

One arrives at the expected conclusion that first-order stations provide the bulk of the A records, although that category contains even third-order stations, and of these, privately maintained ones are in a majority. If at such a station the zeal of the observer may on rare occasions lead him to record as snow what is not snow, this will be balanced by the equally rare occasions when he misses something. One private record contributed by three observers was found to contain an A −, an A, and then a D record. So in utilizing the records, care must be taken also to investigate the responsible authority. School records are generally of lower category, but one or two not included in the analysis are known to be good. University records are variable and may be up to A −. Municipal records are variable, and both A and D records are found. One A record from an airfield became a B when the station assumed third-order status. C and D records are absent from areas of small average annual totals of snow-days. These are mainly in the south-west, where snow compels attention and recording. Moreover since it is mainly Eqs. (1), (2), and (5) which apply to them, with small values of the constant term, there is evidence of a lower frequency of small amounts of sleet in showers of rain with maritime polar air than there is with similar showers at stations of more frequent snowfall further north. It is thus possible for observers of equal opportunity and vigilance situated in different areas to compile records of different categories.

6. Use of the results

The difficulties causing the problem investigated here arise from the fact that no better definition of a day of snow has yet been found. It has been suggested (Ashmore 1947) that if it were possible, in addition, a further definition could be adopted, such as at least 0·01 in of precipitation in the form of snow (I believe something of this sort is done in certain countries abroad) but for the necessary closeness of definition instrumental aid is necessary, and it is not certain how this could be applied to sleet. At present, therefore, the compiler of a map of the average annual number of snow-days must accept the current definition as his standard, which involves using A records and correcting others to the A standard. A map by Manley (1940) was produced from A – and B stations mostly, with a few of category C, with a proviso that if records from A stations had been used, the values would have been from 10 to 25 per cent higher. From the equations given above it is found that this is roughly the case, except that 25 is rather a high figure. An amended map would therefore retain the broad features but would show somewhat higher values. Such a map should be able to show average annual values accurate to about a day.

The foregoing assumes that one knows beforehand in which category to place a station. Table I should help in this matter, and sufficient additional information can be obtained from familiarity with the *Monthly Weather Report*, from the *Introduction* to that report, or better still, by personal acquaintance with the station.

Acknowledgment

Many thanks are due to Professor Gordon Manley for his frequent help, and to Dr. John Glasspoole for helpful criticism.

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551.511 : 512.9

A note on vector operations and coordinate systems

By L. SHERMAN

Department of Meteorology, Florida State University, U.S.A.

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In recent years meteorologists have given considerable attention to the concepts of divergence and vorticity. The careful reader of the literature will have noticed that from time to time some confusion about the terminology occurs. Although the author is aware that nothing in this note is essentially new, he feels that it might be useful to call attention to the distinctions between $\nabla \cdot \mathbf{V}$, $\nabla_H \cdot \mathbf{V}$, $\nabla \cdot \mathbf{V}_H$ and $\nabla_H \cdot \mathbf{V}_H$ and to parallel ones between $\nabla \times \mathbf{V}$, $\nabla_H \times \mathbf{V}$, $\nabla \times \mathbf{V}_H$ and $\nabla_H \times \mathbf{V}_H$ and to make a plea for consistent terminology and notation.

While some of these quantities are distinct even for the case of a 'flat' earth, a large part of the difficulty stems from the fact that in meteorology we customarily deal with essentially spherical coordinate curves (whose unit tangent vectors $\mathbf{i}$, $\mathbf{j}$, and $\mathbf{k}$ are directed to the east, north and zenith) but with linear wind components ($u = dx/dt$ = speed in the $\mathbf{i}$ direction measured in units of length/time, v and w). One solution would be the adoption of a proper coordinate system, spherical co-ordinates (λ = longitude, ϕ = latitude and r = radial distance) or one of the systems recently suggested by McVittie (1948, 1951). Another solution is to retain the system $\mathbf{i}$, $\mathbf{j}$, $\mathbf{k}$ and x, y, z but take into account the space variations of the unit vectors.

From the geometry of the meteorological coordinate system one may readily compute the spatial derivatives of these vectors; they are listed in Table I. Note that by $\delta/\delta x$ we mean $\mathbf{i} \cdot \nabla$, etc. ($\nabla \equiv \mathbf{i}\,\delta/\delta x + \mathbf{j}\,\delta/\delta y + \mathbf{k}\,\delta/\delta z$).

TABLE I. SPATIAL DERIVATIVES OF $\mathbf{i}$, $\mathbf{j}$, AND $\mathbf{k}$

$\dfrac{\delta \mathbf{i}}{\delta x} = \dfrac{\tan\phi}{r}\mathbf{j} - \dfrac{1}{r}\mathbf{k}$	$\dfrac{\delta \mathbf{j}}{\delta x} = -\dfrac{\tan\phi}{r}\mathbf{i}$	$\dfrac{\delta \mathbf{k}}{\delta x} = \dfrac{\mathbf{i}}{r}$
$\dfrac{\delta \mathbf{i}}{\delta y} = 0$	$\dfrac{\delta \mathbf{j}}{\delta y} = -\dfrac{1}{r}\mathbf{k}$	$\dfrac{\delta \mathbf{k}}{\delta y} = \dfrac{\mathbf{j}}{r}$
$\dfrac{\delta \mathbf{i}}{\delta z} = 0$	$\dfrac{\delta \mathbf{j}}{\delta z} = 0$	$\dfrac{\delta \mathbf{k}}{\delta z} = 0$

Now,
$$\nabla \cdot \mathbf{V} = \mathbf{i} \cdot (\delta u/\delta x\,\mathbf{i} + u\,\delta \mathbf{i}/\delta x) + \mathbf{i} \cdot (\delta v/\delta x\,\mathbf{j} + v\,\delta \mathbf{j}/\delta x) + \mathbf{i} \cdot (\delta w/\delta x\,\mathbf{k} + w\,\delta \mathbf{k}/\delta x) + \mathbf{j} \cdot (\delta u/\delta y\,\mathbf{i} + u\,\delta \mathbf{i}/\delta y) + \ldots .$$

Then,
$$\nabla \cdot \mathbf{V} = \delta u/\delta x + \delta v/\delta y + \delta w/\delta z - v \tan\phi/r + 2w/r \,. \qquad (1)$$

Similarly one may obtain
$$\nabla \times \mathbf{V} = (\delta w/\delta y - \delta v/\delta z - v/r)\,\mathbf{i} + (\delta u/\delta z - \delta w/\delta x + u/r)\,\mathbf{j} + (\delta v/\delta x - \delta u/\delta y + u \tan\phi/r)\,\mathbf{k} \qquad (2)$$

Formulae (1) and (2) apply to any vector and are usually sufficient for most operations. Thus, for example, the Laplacian $\nabla^2 F = \nabla \cdot \nabla F = \nabla \cdot (\mathbf{i}\,\delta F/\delta x + \mathbf{j}\,\delta F/\delta y + \mathbf{k}\,\delta F/\delta z)$ and applying Eq. (1)
$$\nabla^2 F = \frac{\delta^2 F}{\delta x^2} + \frac{\delta^2 F}{\delta y^2} + \frac{\delta^2 F}{\delta z^2} - \frac{\tan\phi}{r}\frac{\delta F}{\delta y} + \frac{2}{r}\frac{\delta F}{\delta z}$$

Care must be taken to remember that these spatial derivatives are not commutative. Thus

$$\frac{\delta}{\delta x}\left(\frac{\delta}{\delta y}\right) = \frac{1}{r\cos\phi}\frac{\partial}{\partial\lambda}\left(\frac{1}{r}\frac{\partial}{\partial\phi}\right) = \frac{\delta}{\delta y}\left(\frac{\delta}{\delta x}\right) - \frac{\tan\phi}{r}\frac{\delta}{\delta x}$$

Similarly $\delta/\delta x\,(\delta/\delta z) = \delta/\delta z\,(\delta/\delta x) + 1/r\,(\delta/\delta x)$, etc.

One can define $\nabla_H = \mathbf{i}\,\delta/\delta x + \mathbf{j}\,\delta/\delta y$. Then, repeating Eq. (1):

$$\begin{aligned}
\nabla\cdot\mathbf{V} &= \delta u/\delta x + \delta v/\delta y + \delta w/\delta z - v\tan\phi/r + 2w/r\\
\nabla_H\cdot\mathbf{V} &= \delta u/\delta x + \delta v/\delta y \qquad\quad - v\tan\phi/r + 2w/r\\
\nabla\cdot\mathbf{V}_H &= \delta u/\delta x + \delta v/\delta y \qquad\quad - v\tan\phi/r\\
\nabla_H\cdot\mathbf{V}_H &= \nabla\cdot\mathbf{V}_H \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad (3)
\end{aligned}$$

and, repeating Eq. (2) and writing ζ for the vertical component of the vorticity:

$$\begin{aligned}
\nabla\times\mathbf{V} &= \left(\frac{\delta w}{\delta y} - \frac{\delta v}{\delta z} - \frac{v}{r}\right)\mathbf{i} + \left(\frac{\delta u}{\delta z} - \frac{\delta w}{\delta x} + \frac{u}{r}\right)\mathbf{j} + \zeta\mathbf{k}\\
\nabla_H\times\mathbf{V} &= \left(\frac{\delta w}{\delta y} \qquad - \frac{v}{r}\right)\mathbf{i} + \left(\quad - \frac{\delta w}{\delta x} + \frac{u}{r}\right)\mathbf{j} + \zeta\mathbf{k}\\
\nabla\times\mathbf{V}_H &= \left(\quad - \frac{\delta v}{\delta z} - \frac{v}{r}\right)\mathbf{i} + \left(\frac{\delta v}{\delta z} \qquad + \frac{u}{r}\right)\mathbf{j} + \zeta\mathbf{k}\\
\nabla_H\times\mathbf{V}_H &= \qquad\qquad - \frac{v}{r}\,\mathbf{i} \qquad\qquad + \frac{u}{r}\,\mathbf{j} + \zeta\mathbf{k} \qquad\quad (4)
\end{aligned}$$

The sets of Eqs. (3) and (4) explicitly exhibit the distinctions mentioned at the outset of this note. The reader will be aware that if the terms involving $1/r$ are omitted, the formulae (3) and (4) will reduce to those applicable to a 'flat' earth. Notice that even in this case some distinctions will yet remain. Thus, for example, $\nabla\times\mathbf{V}_H \neq \nabla_H\times\mathbf{V}_H$. Note also that for a spherical earth, care must be taken in operating with ∇_H; thus one cannot transfer the property $\nabla\times\nabla\phi = 0$ to the operator ∇_H.

One may readily derive tables analogous to Table I for other coordinate systems. As an example we show in Table II the spatial derivatives $\mathbf{t}\cdot\nabla$, $\mathbf{n}\cdot\nabla$, $\mathbf{k}\cdot\nabla$, of the unit vectors $\mathbf{t}$, $\mathbf{n}$, $\mathbf{k}$, associated with the quasi-natural coordinate system. Here c and ψ are the speed and direction (as ordinarily measured in meteorology) of the horizontal part of the wind; $\mathbf{t} = \mathbf{V}_H/c$; $\mathbf{n} = \mathbf{k}\times\mathbf{t}$; s and n are lengths measured in the $\mathbf{t}$ and $\mathbf{n}$ directions respectively, and the remaining quantities have already been defined. Table II then follows from $\mathbf{t} = -\sin\psi\,\mathbf{i} - \cos\psi\,\mathbf{j}$, and Table I.

TABLE II. SPATIAL DERIVATIVES OF THE UNIT VECTORS IN THE 'QUASI-NATURAL' COORDINATE SYSTEM

$\frac{\delta\mathbf{t}}{\delta s} = -\left(\frac{\delta\psi}{\delta s} + \sin\psi\frac{\tan\phi}{r}\right)\mathbf{n} - \frac{1}{r}\mathbf{k}$	$\frac{\delta\mathbf{n}}{\delta s} = \left(\frac{\delta\psi}{\delta s} + \sin\psi\frac{\tan\phi}{r}\right)\mathbf{t}$	$\frac{\delta\mathbf{k}}{\delta s} = \frac{\mathbf{t}}{r}$
$\frac{\delta\mathbf{t}}{\delta n} = \left(-\frac{\delta\psi}{\delta n} + \cos\psi\frac{\tan\phi}{r}\right)\mathbf{n}$	$\frac{\delta\mathbf{n}}{\delta n} = -\left(-\frac{\delta\psi}{\delta n} + \cos\psi\frac{\tan\phi}{r}\right)\mathbf{t} - \frac{1}{r}\mathbf{k}$	$\frac{\delta\mathbf{k}}{\delta n} = \frac{\mathbf{n}}{r}$
$\frac{\delta\mathbf{t}}{\delta z} = -\frac{\delta\psi}{\delta z}\mathbf{n}$	$\frac{\delta\mathbf{n}}{\delta z} = \frac{\delta\psi}{\delta z}\mathbf{t}$	$\frac{\delta\mathbf{k}}{\delta z} = 0$

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O.S.T.I.V. 1952; meteorology and gliding

The Organisation Scientifique et Technique Internationale du Vol à Voile held its fourth congress in the rooms of the Real Aero Club de España at Carbachel Alto airfield outside Madrid during the first part of July at the time of the fourth World Gliding Championships. The object of the congress was to review progress since the last meeting two years previously and consider probable lines of progress in the near future. All aspects of motorless flight were considered and over forty papers were presented, to be later published with discussion. In this report only those which have a bearing on the trend of meteorological thought or with the usefulness of gliders as instruments of scientific research will be mentioned and these will be described as seen through the eyes of one who is very much interested and therefore by no means impartial. A more balanced review of the importance of various papers will be possible when they are published : only a very few were available in manuscript at Madrid and so this report is based on a single hearing of most of them.

W. Georgii described, in two papers, how waves in the lee of the Andes, of length long enough for the earth's rotation to be important in their control, can contain thunderstorms in their crests. The dynamic effect of these waves may be aided by the semidiurnal pressure surge which can cause bursts of Atlantic air to undercut the westerly airstream that flows over the mountains all the way from Brazil to Tierra del Fuego. There is a rhythm in the occurrence of föhn winds at Mendoza (Argentine) where these studies were made and the cold air appears aloft at first after about two days of föhn winds : this produces instability and the cold air then comes in at the surface. The papers were illustrated by aircraft and glider-flight reports which show a considerable phase variation with height of the waves. Owing to language difficulties these papers were not easy to follow, particularly because the explanations were not always acceptable; but it is plain that Prof. Georgii's acute observation has lighted on an interesting and important phenomenon which is well worth meteorologists' attention.

A. H. Yates described a selection of wave flights by gliders in Britain. Conscious of what was to follow from the Alps and California his audience were delighted to hear of the almost universal occurrence of waves in Britain. He showed clearly that it does not require a mountain range of several thousand metres height to produce a glider pilot's paradise, British pilots had already soared to ten times the hill height and in many cases they did not go higher only because they were not prepared or because of the onset of darkness. His material was of great interest to the mathematicians who, equipped only with perturbation theory and linearised equations cannot begin to cope with the problems raised by airflow over the Alps, and Sierra Nevada in California. Some of the audience went away thinking that all the glider pilot had to do was to get in the air and there were plenty of waves all over Britain to soar in all the time.

Frau Vieweg-Pielsticker described her own work and that of H. Wächter in studying föhn waves in the eastern Alps and Taunus mountains. She described briefly the meteorological conditions and showed many beautiful pictures, both still and time-lapse, of the clouds. Of particular interest was the great wall of cloud on the lee side of Gross Glöckner close to which Klöckner soared to the (then record) altitude of 11,410 m, and some pictures of cirrus streaming as if it were smoke from an invisible line of chimneys over the mountain peaks.

M. Castans Camargo, who was the official Spanish forecaster for the gliding contest, described waves in the lee of the Pyrénées in which gliders had soared. The waves occurred when a depression crossed France and slowed down in the Gulf of Genoa and the cold front crossed Spain. The wind between 2,500 m and 5,000 m blows from the N. across the Pyrénées but the surface temperatures indicate that the air does not flow down the south side but flows into the Ebro valley from the NW. The air in this lower current is turbulent but above it the air is smooth. Cumulus forms in the lower layer in a line parallel to the Pyrénées and sometimes a line of lenticulars forms in the airstream blowing across at higher levels. In the lower current the thermal upcurrents appear to form two parallel lines under one wave crest in the upper current, and because the waves appear to have a stronger vertical velocity on the windward side it is thought that the wave crests are found further downwind at greater height. Though more observations are needed, the interpretation of the observations was all the

more convincing because the temperature sounding for the Ebro valley as measured by glider agreed with the Bordeaux sounding translated to the Ebro valley according to the picture given. Three wave crests were to be observed in typical cases.

F. Höhndorf then exhorted the theoreticians not to pay too much attention to the details of their hill profiles because of local thermal and frictional variations.

We then came to a discussion of the mathematical methods available and several new lines of attack were described. All were aware of the necessity to advance beyond the now classical papers of Lyra and Queney which discussed the flow of a uniform stream over a two-dimensional ridge. The differential equation governing the motion is linear and second order (using perturbation theory). That the ground profile provided one boundary condition all were agreed, but the second condition necessary for complete solution was disputed, and must be regarded as unsettled. This opinion was not universal for many of those present regarded the matter as settled in their own way (by arguments every one of which was open to question in view of points raised at other times in the general discussion). It must be admitted that the actual constraints which determine the flow are not properly understood and the meteorologist must look with scepticism on arguments which seem to satisfy the mathematicians and depend on the state of the flow at $+$ or $-\infty$, or at infinite height. Much play was made of the way in which the decrease in density with height amplifies the waves at high levels while sound waves, which enter into the equations to an equal extent in the same terms, were ignored; and while it was evident that the discussion of steady two-dimensional flow was unreal it was persisted in largely because of the mathematical difficulties. It is worth while therefore to mention first those aspects of the papers which made a closer approach to reality.

P. Queney showed that damping by viscosity was negligible and discussed how a wave was damped in time if there was uniform shear. He showed that, contrary to popular belief, shear was a damping agent while curvature of the velocity profile was an agent in instability. In particular a discontinuity of shear was unstable.

R. S. Scorer concentrated mainly on the properties of trains of waves which in two-dimensional flow are undamped but whose amplitude dies out upwards. It became evident that he was concerned more than anyone else with these because they occur, so it seems, more in Britain than elsewhere. This ' land of small perturbations ' provided almost daily examples of parallel bars of cloud and orographic clouds at great height, and he discussed how conditions in the friction layer often determined the nature of the flow at great heights. He also made the only excursion into three space dimensions. A paper by E. Hoiland provided the only attack on the problem of stable unsteady flow. His model was very simple but he avoided the abstractions of infinity by starting the flow from rest. He obtained waves on the lee side of the obstacle only.

G. Lyra extended his classical paper to include curved velocity profiles, and J. Zierep considered some special profiles (his mathematics is always neat) while H. Merbt gave a new derivation of Eliassen's result in that passing over one edge of a plateau a jet in the velocity profile is produced. He also explained a new technique for dealing with the classical equations by using Mathieu functions. M. G. Wurtele produced undamped waves in which one wave length dominated at low levels and a longer wave at higher levels, while a paper by de Ver Colson showed how to produce lee waves using simple mathematics.

F. Rossman considered the effect of condensation on these theoretical solutions and concluded that in the windward side of a wave cloud the vertical velocities would be greater than in the lee side.

J. Küttner not only read several of his own papers and presented those of some other people who were absent, but also spent many hours translating papers and discussions to and from English and German. W. Klemperer also undertook interpreting and he and Küttner made possible discussions on a scale that was most valuable. Between them they described the Sierra Wave Project and showed a film of it. Not only is this the biggest glider project so far, designed to investigate a wave phenomenon unequalled in magnitude elsewhere, but it is trying out and using techniques which will be of great value in other projects. One aspect of particular interest at this conference was the opportunity it provided of getting a research glider up to the tropopause by soaring in the wave : in this way it is possible to investigate the phenomenon of clear-air turbulence which often occurs on the poleward side of a jet stream, for a wave in which gliders can soar also occurs under these conditions. The

turbulence is at times uncomfortably severe for the gliders. Balloon measurements, described in a paper by de Ver Colson, showed enormous variations in horizontal wind and it was not certain whether these were mainly space or time variations.

The conditions under which unstable waves could grow and stable ones be propagated in a duct near the tropopause where the wind velocity is a maximum came under discussion, and the controversies were not resolved owing to pressure of time and the paucity of observations. It was clear that here was a subject whose importance will grow. After his own discussion Küttner described a paper by G. R. Trefry who presented some observations from Australia including one of a shear of 60 kt in a depth of 1,000 ft. He claimed theoretical basis for his theory in an early paper of Scorer who, however, disputed the ideas when presenting a note by A. E. Parker. Parker and Trefry each laid emphasis on the importance of the curvature of the velocity profile as a possible source of instability, and therefore of turbulence. They claimed some observational confirmation for their ideas, and the curvature is evidently more important than the formerly popular Richardson number, but the problem is more complex than they suggest. Küttner's own discussion of the theory was more fundamental and it is hoped that he will soon publish the observations to which he has applied it for he has faced the problem of having to differentiate the observations two or three times.

A paper by J. Förchtgott described how the flow over a two-dimensional ridge depends on the depth of the ' streaming layer.' Various kinds of streaming were described and one type was compared with the Kàrmàn vortex street. The main distinction was between the ' rotor streaming ' of the lower layers and the laminar motion above. Chaotic and turbulent flow were found in the lee of high ridges with a very shallow streaming layer, particularly when there was a sharp decrease of velocity at the top of this layer. This study was based on many sets of observations from gliders and a close network of meteorological stations in the mountains of Bohemia. A touch appropriate to the occasion was provided by Küttner's description of his wave flight in the Rockies in which he covered 600 km in less than four hours.

The discussions of cumulus convection were less extensive. F. Höhndorf was concerned mainly with the release of thermal bubbles from orographic features, a paper by H. R. Byers reviewed the *Thunderstorm Project*, H. Koschmieder described observations near Berlin in 1939-40 on thunderstorms, and W. Georgii told about high-level thunderstorms of a kind not met with outside Argentina which depended on the wind systems to the east of the Andes. In an important paper by F. H. Ludlam, freely presented by R. S. Scorer, a cumulus cloud was envisaged as an assemblage of bubbles which waste away as they ascend, the cloud consisting largely of their wakes. The structure of hailstones was explained and reference was made to Schonland's observations on lightning flashes. The ideas briefly described here will be of increasing importance in the near future for they are guiding the research using gliders that is being carried out in Britain.

The conference as a whole covered a branch of meteorology that scarcely receives adequate attention in the literature, namely the study of vertical motions on a scale sufficient to affect gliders. It was clearly shown not only that most of our knowledge has been derived from the experience of glider pilots but also that they will continue to be the main source of information. The international competitions themselves took place under weather conditions which were in themselves worthy of special study. Without the gliders very little attention would have been paid to them and very little observational material, e.g., on downdraughts in thunderstorms, would have been available. There was ample opportunity for informal discussion. For some it was a reunion, but for most it was the first opportunity to talk with those whose works they had read.

R.S.S.

A statistical model for water-vapour absorption

By R. M. GOODY

(Read 21 May 1952. See *Q.J.*, **78**, p. 165)

Discussion

Mr. A. R. Curtis : I should like to make some remarks on the application of Dr. Goody's statistical model to varying pressure paths. I shall deal only with the form of Dr. Goody's transmission function which results from taking a particular probability distribution of line intensities in the band. This distribution, given by $P(k) = 1/\sigma \exp(-k/\sigma)$, was found by Goody to work well for water vapour; he tells me that there is reason to believe that a different distribution should be used for ozone, and if this proves to be the case the methods I am going to describe can be easily adapted.

For this case, the transmission function can be simplified, giving

$$\log_e \bar{T} = -\frac{1}{\delta}\int_{-\infty}^{\infty} \frac{\sigma m S(\nu, \alpha)}{1 + \sigma m S(\nu, \alpha)}\, d\nu \qquad (1)$$

If the line shape is Lorentz, we have

$$S(\nu, \alpha) = \frac{\alpha}{\pi}\frac{1}{\nu^2 + \alpha^2} \qquad (2)$$

and the transmission is

$$\bar{T} = \exp\left[-\frac{\pi\alpha}{\delta}\frac{x}{(1+x)^{1/2}}\right] \qquad (3)$$

where $x = \sigma m/\pi\alpha$.

Along a varying pressure path, however, the line shape is a superposition of Lorentz shapes with different values of α, and is not itself Lorentz. I first conjectured that for all normal conditions in the atmosphere an expression of the form (3) might still be valid if some sort of average value was substituted for α. Assuming that this is true, it is easy to prove that the value chosen must be the average value weighted with amount of absorbing matter, i.e.,

$$\bar{\alpha} = \int \alpha dm / \int dm \qquad (4)$$

Accordingly, various distributions of absorbing matter were assumed, and the Goody transmission function computed, (i) using the exact line shape, and (ii) using a Lorentz line shape with $\bar{\alpha}$ substituted for α. At first the results were very encouraging. (see Fig. 1). With the normal atmospheric water-vapour distribution in mind, the mixing ratio (density of absorbing gas/density of air) was chosen first to be constant and then to be proportional to pressure, i.e., to decrease with height. The results were excellent, the difference between the two expressions never exceeding one per cent even when the distribution extended over the whole height of the atmosphere. As a final check, it was decided to carry out the computations assuming two slabs of absorbing matter concentrated at two different heights. Again the agreement was good when most of the matter was at the higher pressure, but became poor when most of it was at the lower pressure. Since the concentration of ozone does increase with height, this suggested that the approximation might not be satisfactory for this gas; and changing the constants to fit an ozone absorption band gave definitely bad agreement between the approximate and exact transmission functions. This was disappointing, but it seemed that the bad result in this case might be due to the severe distribution chosen, and for some time I tried to prove that for all plausible atmospheric distributions of ozone the approximation of using Eq. (3) with a mean line width given by Eq. (4) could be used. In any case this approximation was established for water vapour.

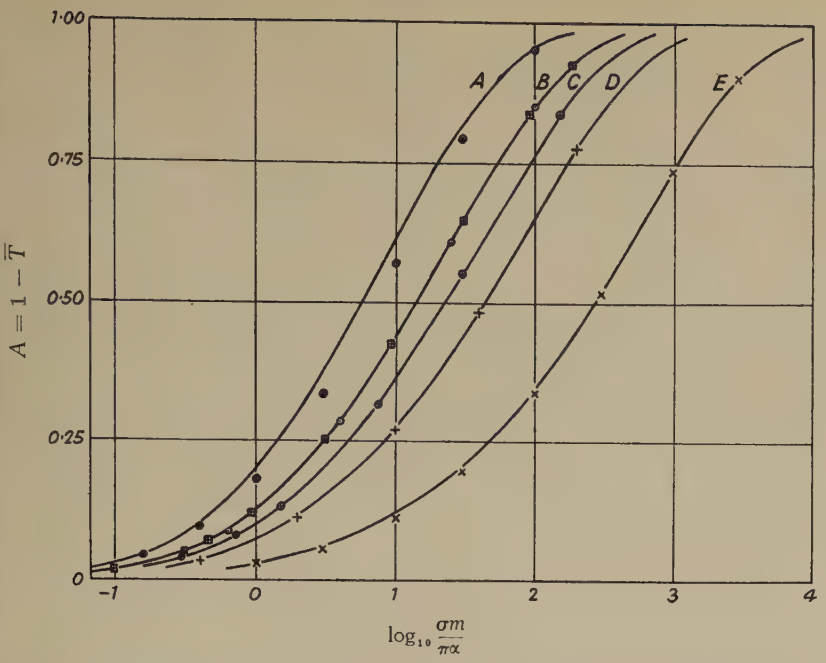

Figure 1. The approximation of a mean line width. The points show the exact Goody transmission functions for various distributions of absorbing matter with pressure, and the curves show the approximate functions obtained by taking a Lorentz line shape with the mean line width $\alpha = \int \alpha dm / \int dm$.

Curve B. Water-vapour rotation band, $p = 0{\cdot}9$ atm. ⊞, constant mixing ratio over a range e : 1 in pressure, $\bar{p} = 0{\cdot}9$ atm. ⊙, two slabs of absorbing matter, m_1 at 0·1 atm and 8 m_1 at 1·0 atm.

Curve C. Water-vapour rotation band, $p = 0{\cdot}7$ atm. ⊕, mixing ratio proportional to p over the whole height of the atmosphere, $\bar{p} = 0{\cdot}7$ atm.

Curve D. Water-vapour rotation band, $\bar{p} = 0{\cdot}5$ atm. +, constant mixing ratio over the whole height of the atmosphere, $p = 0{\cdot}5$ atm.

Curve E. Water-vapour rotation band, $p = 0{\cdot}2$ atm. ×, two slabs of absorbing matter, m_2 at 1·0 atm and 8 m_2 at 0·1 atm.

Curve A. Ozone 9·6 μ band, $p = 0{\cdot}05$ atm. ⊗, a distribution similar to × but with pressures divided by 4.

Note the moderate systematic deviations from curve E and the large systematic deviations from curve A.

However, I discovered a theoretical reason why this approximation should be less accurate for concentrations increasing with height. If the line shape for such distributions is compared with the Lorentz shape assumed in the approximation, the two agree in the wings; but the actual line has a more intense centre and, since the areas are the same, less intense shoulders. The more intense centre is not very serious, because for small quantities of absorbing matter the absorption is linear and only the area under the line is important, while for somewhat larger quantities the centre is fully absorbed anyway. But the less intense shoulders mean that, for a given quantity of absorbing matter sufficient to black out the line centre, the width of the blacked-out part will be smaller for the true line shape than for the approximation. The latter will therefore overestimate the absorption in this region, which is exactly the result found from the computations.

Thus, while the simple approximation of taking a mean line width can be used for water vapour, a more accurate treatment for ozone is necessary. As this must clearly be based on the exact line shape and yet not entail too laborious computations, the use of a graphical method suggested itself. The problem is essentially one of evaluating integrals, whose integrands contain a parameter, for many values of the parameter.

So far I have designed three charts for the successive stages in computing the transmissio of a given distribution of absorbing matter. The first computes the line shape from th given distribution, assuming Döppler broadening unimportant; the second modifies th computed shape to allow for Döppler broadening; and the third computes the Goody trans mission function for this line shape. These charts work on the principle of superposing sheet of transparent graph paper in different positions on another to represent different value of the parameter, and carrying out the integration by a counting squares method. I hop shortly to publish details of the construction and use of these charts.

Dr. R. S. Scorer : By ordinary meteorological standards the curves which Mr. Curti showed seem to be a very good fit to the points and not very bad as he seemed to judge. Ca Dr. Goody explain why such small differences in the emission properties of elements tha are relatively rare in the atmosphere should be important ? If ozone and water vapour behave like Mr. Curtis's formulae, would the temperature structure of the atmosphere and the hea gains and losses by the troposphere be significantly different from what they are ? Is ou weather precariously poised on the shape of an emission line ?

Dr. R. M. Goody (*in reply*) : I was extremely glad to hear that Mr. Curtis has ha some success in his mathematical treatment of radiative transfer. This is a problem whic requires a high degree of mathematical skill and satisfactory solutions will not be forthcomin until it receives expert attention.

Dr. Scorer raises a great many complicated issues. The present paper is a purely technica contribution and I had hoped that I might be forgiven the omission of an introduction o the grounds that I have recently collaborated with Dr. Robinson in a review of the whol subject for the *Quarterly Journal*. In the main Dr. Scorer will find the answers that he require in that review, but I will add the following brief replies :

(i) Minor constituents are important because the main constituents do not absorb and because there is quite enough of them to make the atmosphere opaque in their principa absorption bands.

Accurate absorption data are required for two main reasons. The evaluation of radiative heating involves two differencing operations and errors in the second or third significan figures of the absorption can be important. Secondly it is usually necessary to extrapolate so far from laboratory data that the methods adopted have to have a very sure foundation.

(ii) The second question continues from the first. We shall not know what error i involved in various approximations until accurate methods have been evolved as a standard It is unlikely that direct measurements can give accurate results away from the earth's surface and the mathematical equations do not lend themselves to estimates of error. When accurate methods have been evolved it will be possible to test simplifications, and no doubt many o them will be adopted. I note that recent papers by Kaplan in the *Journal of Meteorology* also take this point of view.

(iii) If absorption lines in the lower atmosphere had a different shape from that which they have, then the weather would not be what it is. The connection between the distribution of heat sources and sinks and the resultant hydrodynamic motions is the second fundamenta problem of meteorology (the first, and by far the simpler, being the one under discussion) and until it is solved I cannot say whether or not the poise is precarious.

An essay on the general circulation of the atmosphere over South-East Asia and the West Pacific

By B. W. THOMPSON

(Read 21 May 1952 (presented by Mr. G. S. P. Heywood). See Q.J., 77, p. 569)

Mr. C. S. Durst : During the war when operations were being planned against Japan, we had to consider the synoptic climate of China and, I believe, Dr. Hare who was then working with me, came to much the same conclusions as Mr. Thompson.

The limiting position of the southerly monsoon over China was recognized with its air drawn partly from the Bay of Bengal and partly from the Pacific. The intertropical front drawn through the Himalayas is clearly fictitious and Petterssen shows his intertropical front as discontinuous between eastern China and India.

I well remember how satisfactory it was to find the deductions on Chinese climatology which Dr. Hare had produced were corroborated by the findings of Mr. A. Lu who was simultaneously and independently working on the same subject. I am rather surprised that Mr. Thompson has not referred to the work which has been done on this subject by Mr. H. C. Huang, Mr. A. Lu and Mr. Chang Wang Tu.

Mr. J. S. Sawyer : I do not understand the author's objection to the conventional summer position of the intertropical front, because the author's own diagrams showed convergence zones in much the same position. The difference seems to be mainly one of terminology. There seems to be some advantage in retaining the name intertropical front for the air-mass boundary which extends from the Tibetan region eastward across China in summer. Viewed on the world-wide scale this front is seen to be associated with the belt of low pressure which circles low latitudes and which is displaced into Asia in summer by the monsoon system. It could also be said that this front lay close to the ' thermal equator.'

Mr. L. C. W. Bonacina : In reading this paper it struck me that it would be well if the author attempted to correlate his new representations with the older views before they appear more succinctly in the text-books – particularly, perhaps, in regard to the tradition that the copious supplies of vapour contained in the SW. Monsoon currents must come from south of the equator. It would, I feel, be sometimes difficult to distinguish between a ' front ' and a ' convergence.'

Dr. A. G. Forsdyke : I think there is little doubt that a large part of the W. to SW. air stream which covers the Arabian Sea in the northern-hemisphere summer and constitutes the SW. monsoon of India does originate in the southern hemisphere. At this season there is a continuous and almost steady gradient of pressure between the sub-tropical anticyclone centred to S. of Mauritius and the seasonal low-pressure centre over NW. India. Studies of such surface and upper-air observations as were available from the African mainland, Madasgascar and the few island stations during the years 1943-5 indicate that the air flow is roughly such as would be expected from this pressure distribution, namely a steady SE. to ESE. current down to fairly low latitudes in the southern hemisphere swinging through south as it crosses the equatorial belt. This regime prevails over the whole region between the African coast and the longitude of the Seychelles. Along the coast of E. Africa the air stream becomes southerly or even slightly west of south before reaching the equator : I consider that this is a deviating effect of the African plateau. East of the Seychelles observations are very sparse, but near the equator it seems that the pressure gradients are very weak, and there is no evidence of any pronounced transfer of air between the hemispheres.

Mr. R. G. Veryard : From my forecasting experience in tropical regions I would say that it is doubtful whether there is a continuous intertropical front or intertropical convergence zone as depicted by Sawyer, any more than there is a continuous polar front. Over the continents there is certainly what one might call a ' humidity front ' and I would say that the moisture-laden air which brings summer rainfall to the Sudan has originated from the Indian Ocean south of the equator, as Dr. Forsdyke has already suggested in respect of East Africa.

On the other hand, it is questionable whether the dry air on the north side of this 'humidity front' can be regarded as being of polar origin. It may have been at the poles during its life history but, judging from the temperatures which I have experienced in this air, it has become sub-tropical continental air *en route*, and it would be misleading to call the front a polar front.

Regarding the mysterious 'jump' which this 'humidity front' appears to make, I suggest that a possible explanation is that, using the cross-section of Sawyer's the slope of the plane of separation between the moist relatively cool air to the south and the dry relatively hot air to the north is very small and that, as the result of slight variations in turbulence, moister/drier air may be brought to the surface over a large distance in a short time, i.e. the 'humidity front' at the surface may be quickly destroyed at one position but re-created at another as the result of vertical mixing.

Sir Charles Normand: I feel very old when I read in this paper about the 'traditional' intertropical front of north India, which is a relatively recent and perhaps temporary nomenclature. I have been brought up on the nomenclature of the monsoon trough. As the history, properties and vertical extent of the air masses around the trough vary from day to day with various factors, including the position of the trough, the term 'intertropical' front must be a disputable point on climatological or monthly maps.

I note that the author prefers to regard the summer N. Indian westerlies as being of north hemispheric origin by the time they reach China. That might be more true of the air mass as a whole than of its moisture, which is the important property. There is little doubt that the Indian monsoon rainfall originates mainly as moisture in the southern hemisphere. Much of the moisture in the summer westerlies over south China may also originate south of the equator.

Dr. P. R. Crowe (*communicated*): Having been engaged in the analysis of some of these problems on the basis of surface data I find Mr. Thompson's paper extremely stimulating.

Let us note, however, the dangers inherent in an attempt to replace the objectively-determined mean chart by a subjective estimate of the 'flow distribution most commonly occurring.' If we accept the mode but reject the mean it must be inferred that we recognize the likelihood of considerable differences between them. Yet wide contrast between mode and mean is only possible in a distribution so scattered and so skew that the concept of the typical becomes of very doubtful validity. We should be given some statement of the probability of occurrence of the patterns laid before us.

In particular I have difficulty with the NE. winds at 2,000 ft over the Bay of Bengal in May (Fig. 12), for Admiralty surface charts show a relatively persistent SW. flow at that time. Even on theoretical grounds it is hard to see why there should be an 'Indo-China low' at this season and not another over the much hotter Indian sub-continent.

If the intertropical convergence zone is regarded as broadly delimiting the air masses of the two hemispheres I can see nothing in Mr. Thompson's interesting suggestions as to its nature, simple or multiple, which invalidates such a concept. Would it not be wiser, however, to regard the polar fronts, Pacific and Atlantic, as *converging* upon this zone, rather than as becoming completely identified with it?

I was particularly interested in the references to the equatorial westerlies. In the Indian ocean they are quite clearly recognizable on surface charts, particularly in October, a month to which Mr. Thompson devotes little attention. They remain astride the equator during November and December though with decreasing persistence and strength and are recognizable locally to the south of it until March. Possibly they too are associated with equatorial eddies not unlike those recognized over the East Indies.

Indeed, 'unfortunate myths' arise only through the *misuse* of surface data. If wind persistence and wind strength are given as much attention as wind direction, the great wealth of surface-wind observations, particularly over the sea, is not to be despised. In regard to the probable origins of the Indian monsoon, the absence of a genuine monsoon in northern China, and the undoubted significance of the equatorial westerlies I find Mr. Thompson's conclusions remarkably convergent with my own (*Trans. Inst. Brit. Geogr.*, Nos. 15, 16 and 17 (in the press)).

Mr. W. D. S. McCaffery (*communicated*): It is increasingly evident that forecasters in tropical regions and research workers in tropical meteorology are seeking for models of tropical regimes which will give a closer approximation to the atmospheric circulation than

is given by classical concepts of the intertropical front and the monsoons of the Indian Ocean. Watts (1949) has described the convergence zones of the Malayan area; the paper under discussion supports conclusions concerning the nature of the I.T.F. in the western Pacific previously arrived at by Riehl (1948); Hogan's (1948) analysis of weather in the Indian Ocean is at variance with the classical picture for at least some months of the year. Forecasters in the West Indies are in some doubt as to the true explanation of disturbances which occur from time to time over the Guiana coast and which have previously been attributed to northward fluctuations of an I.T.F. considered continuous in time and space. This paper is welcome as a contribution to the new and improved models which are needed to fit the data now available from tropical areas.

Mr. Thompson asks how far westwards the West Pacific convergence zone extends (Figs. 14, 15, 17 and 18). From about mid-May to September the south-east trades of the southern Indian Ocean terminate in a convergence zone which separates them from equatorial westerlies. This zone, which may be considered as an extension of the W.P.C.Z., fluctuates about a mean position three or four degrees south of the equator, often exhibits considerable activity, but weakens westwards and is seldom in evidence west of about 65° to 60° E. It appears highly probable that only beyond the western end of this zone does a true turning of the south-east trades across the equator take place as a day-to-day feature of synoptic charts. The convergence zone is not necessarily in evidence along its whole length on every chart, but there is no doubt that it must be looked for somewhere near the average position and that it is often in evidence along a length of many hundreds of miles.

It may be pertinent to remark that a rather similar convergence zone is in existence across the Indian Ocean during the period of the NE. monsoon. This usually lies some three degrees or so north of the equator, but weakening, may drift south of the equator to be replaced eventually by a new formation further north. The NE. Monsoon terminates in this zone, which separates it from a belt of equatorial westerlies. Again it is highly probable that only west of about 60° E. is there any true turning of the NE. Monsoon across the equator into the NW. or Cross Monsoon. Fig. 2 of Mr. Thompson's paper shows such a structure at 10,000 ft, but the confergence zone is also in evidence at the surface.

Forsdyke (1949) notes and defines a discontinuity known as the Monsoon convergence zone, but states that much more evidence of its existence is required before it is accepted. I am quite sure that, in so far as the two convergence zones I have described may be called Monsoon convergence zones, there is abundant evidence for their existence.

References

Forsdyke, A. G.	1949	*Geophys. Mem. Met. Off.*, London, **10**, No. 82.
Hogan, J.	1948	*Melbourne Met. Branch Bull.*, No. 40.
Riehl, H.	1948	*J. Met.*, **5**, p. 247.
Watts, I. E. M.	1949	*Malay Met. Ser. Mem.*, No. 3.

Mr. G. S. P. Heywood: I agree with Mr. Sawyer that the point under discussion regarding the intertropical front is mainly one of terminology. Mr. Thompson argues that it is misleading to apply this term to the northern boundary of the summer monsoon in China, since this airstream has normally travelled over a vast stretch of the Indian Ocean north of the equator before crossing Siam and Indo-China to reach the south China coast (see Figs. 14, 15 and 18). By then it can hardly be labelled as southern-hemisphere air. The SW. monsoon of India may have had a fairly short passage from the southern hemisphere, as Sir Charles Normand and Mr. Forsdyke point out; in Hong Kong, on the other hand, we are coming to the conclusion that air of *recent* origin in the southern hemisphere rarely if ever reaches us, although Australian air is said to reach the Philippines at times.

I do not think that the high moisture content of the summer monsoon necessarily indicates a recent origin in the southern hemisphere, as Mr. Veryard and Mr. Bonacina have suggested. The air reaching S. China has had ample opportunity to pick up moisture during its long passage over the uniformly warm waters of the China Sea and the northern part of the Indian Ocean – waters which lie in the region of the thermal equator during summer. In fact this air in Hong Kong has been found to be only very slightly warmer and moister than Pacific air coming our way in summer from the NE. trades.

In reply to Mr. Bonacina's second point, the distinction between a ' front ' and a ' convergence zone ' lies primarily in the method of analysis employed. In extratropical regions, the surface charts, with their well-marked pressure patterns and fronts, provide the material for what Palmer calls a ' definitive analysis.' In the tropics, where pressure gradients are normally slight and fronts rarely exist, meteorologists are coming more and more to use the streamline charts as a basis for analysis. Convergence zones can be located in the tropics; fronts cannot.

Mr. B. W. Thompson (*in reply, communicated*) : It is noteworthy that most comments upon my paper concern themselves with Indian Ocean events, and there is little criticism of the description of conditions in the China Area. It is essentially because my outlook has been that of the Pacific meteorologist that I have rebelled against the intertropical front concept, evolved and maintained to a large extent from the Indian Ocean. Now, however, as a member of the East African Meteorological Department and, with Indian Ocean charts before me, I am, in a much better position to reply to the majority of comments than I otherwise would have been!

I would join issue with Mr. Sawyer on the subject of nomenclature of the intertropical front of the northern summer. There should not be two names for one system; the so-called intertropical front of India links directly with the polar front of the Pacific, which is a continuous entity summer and winter. The Pacific meteorologist can hardly be asked to change his title for a system which is permanent, continuous and most definite from season to season simply because during the summer it is called something else in India. Again I point out that airflows on either side of this summer *Asiatic polar front* are similar over both China and India, and are strictly comparable with those separated by an Atlantic polar front, viz., mainly humid westerlies on the south side and northerlies on the north side. That the air is hot on the north side over India, as remarked by Mr. Veryard, I have not disputed – the air on the north side of a long trailing polar front over the Atlantic may also be non-polar in characteristics. My point is that although viewed from India the system is admittedly unlike what is commonly regarded as a polar front, viewed from the standpoint of Asia a continental summer polar front becomes evident. Furthermore, if the reality of equatorial clockwise eddies is accepted, we have another claimant for the title ' intertropical front " – called by me ' intertropical convergence zone ' as a distinguishing mark. One cannot have two different systems of the same name.

I can see no advantage in ' retaining the name intertropical front for an air-mass boundary which extends from the Tibetan region eastward across China in summer,' when it is called polar front at every other season. Furthermore ' viewed on the world-wide scale,' this system is *not* seen to be associated with the belt of low pressure which circles low latitudes. Instead it is associated with the chain of depressions which extends eastwards from Japan to Alaska in summer, as can easily be envisaged from my Fig. 16. The chain of depressions along the Asiatic polar front over China is normally separated from the equatorial low pressure area of the Pacific by the ridge from the Pacific anticyclone which can be seen extending south-westwards in Fig. 16 and which is generally far more extensive. I can assure Mr. Sawyer that these depressions travel north-east to Japan and beyond; none ever turn into the low pressure area of the equatorial Pacific. Furthermore, over the Pacific, my convergence zones are not ' in much the same position ' as those of the classic picture – they are completely different – and so are the airflows. No, if the intertropical front of summer is to be regarded as the limit of southern hemisphere air, and N. Indian westerlies are to be regarded as south-hemisphere air, then this I.T.F. must extend from Tibet to N. China and then southwards to the equator at Borneo. What it should be called and where it should go from this point is beyond my imagination.

I agree with Dr. Forsdyke and Mr. McCaffery regarding the origin of much or most of the equatorial and N. Indian westerlies, at least at levels below about 8,000 ft, and particularly from my recent experience do I accord with the latter's description of events in the Indian Ocean. Indeed I include a specimen chart, Fig. A, which will suffice to extend my original Fig. 14 to the west. It is of course a surface chart since no upper-air data are available from the Indian Ocean, but it is extremely significant, particularly if the effects of friction are allowed for and the flow at 2,000 ft thereby conceived. Although there is little information from the eastern Indian Ocean on this chart (it is very difficult to produce one chart with the *whole* area well-covered with reports) the western end of the W. Pacific convergence zone

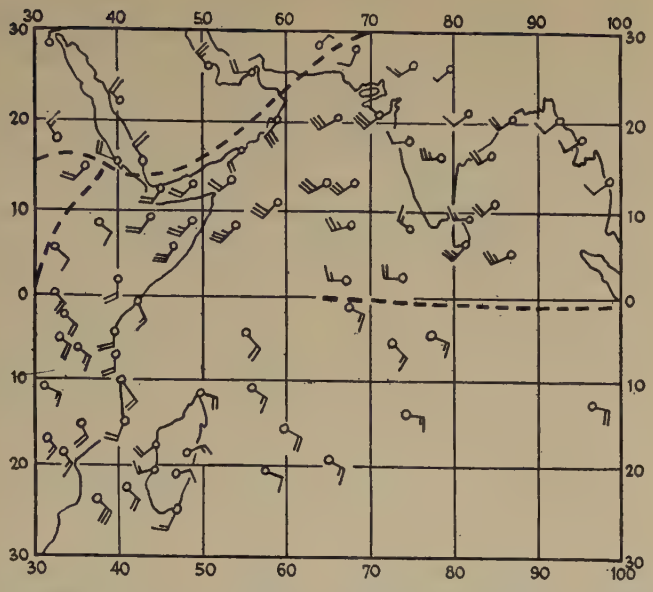

Figure A. 29 June 1952 at 12 GMT. A typical undisturbed condition in the Indian Ocean in the northern summer. Winds in Beaufort force. The Asiatic polar front and the extension of the W. Pacific convergence zone both visible.

is identifiable. Furthermore we have observed a case of an equatorial clockwise eddy on this system in the eastern Indian Ocean west of Sumatra, so that my own query has been answered. The dying out of the W. Pacific convergence zone and the general turning of air east of Africa is exemplified. I would be dishonest to Mr. Sawyer if I did not point out that this large standing 'equatorial clockwise eddy' automatically converts the Indian section of my Asiatic polar front, and his intertropical front, into a very large example of my type of intertropical convergence zone at an equatorial clockwise eddy!

Again, I am forced to agree with Mr. McCaffery regarding the winter condition in the Indian Ocean and in this connection I now realise that my Fig. 1 should be modified. I submit a chart, Fig. B, which exemplifies the surface condition in the Indian Ocean during the northern winter. It will be seen that my equatorial – N. Pacific trades discontinuity of Fig. 2 can indeed be traced westwards to Madasgascar (and beyond) on the surface charts, a fact which I failed to recognize from the sparse information available to me when I prepared my Fig. 1. I do not think however that the generally light equatorial westerlies of the Indian Ocean, in the northern winter months, are very pronounced in the East Indies area at low levels, although there is no doubt of their predominance at 10,000 ft. The southern discontinuity shown in my Fig. B becomes my W. Pacific convergence zone during the northern summer. I find it particularly significant that this discontinuity is so marked a feature east of Madasgascar during the cyclone season since it forms precisely the condition I found in the China Seas and Pacific for the development of typhoons, viz., an east-west discontinuity with westerlies on the equatorial side and easterlies on the poleward side, a flow distribution inherently unstable at the discontinuity where vortices are therefore likely to develop. On 16 Mar. 1952 there were two cyclones and one depression on this discontinuity. The upper wind frequencies on the route Ceylon-Australia given by Hogan (*Bull. Austr. Met. Serv.*, No. 40, 1948) admirably demonstrate the existence of the zonal discontinuities across the eastern Indian Ocean at different seasons.

With regard to Dr. Crowe's remarks upon the objective determination of charts. It is a feature of the tropics and sub-tropics that there are major airflows, according to the season, which, although naturally liable to fluctuations and variation, nevertheless provide the fundamental background to the daily synoptic situations. Thus, although the SW. monsoon of India may vary in strength or extent from day to day or from year to year, and although the rains associated with it may at times even 'fail,' I think it true to say that the airflow 'SW.

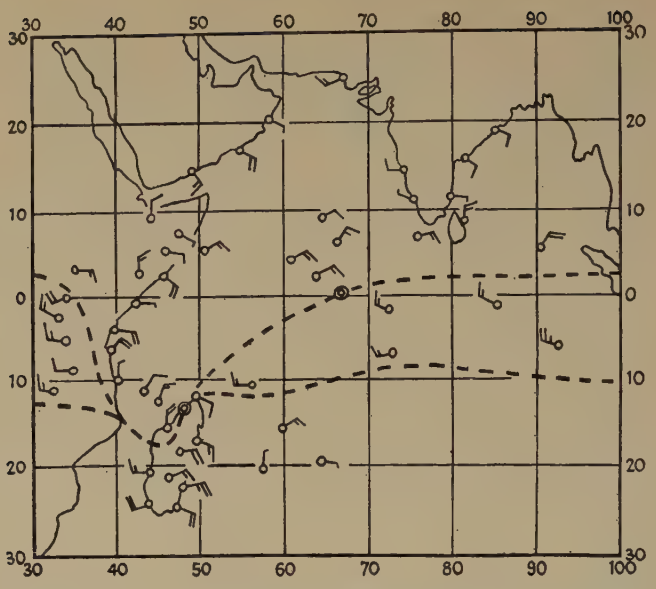

Figure B. 26 Feb. 1952 at 12 GMT. A typical undisturbed condition in the Indian Ocean during the northern winter. This chart considerably modifies my original Fig. 1.

monsoon' has never failed to predominate over India during its appointed season. The synoptic technique aims at detecting these minor fluctuations and developments which profoundly affect the detailed weather. The first essential therefore is to determine the basic pattern, and my charts attempt to locate these broad fundamental air flows over the area concerned. The mode and mean, I think, would prove almost identical – the W. Pacific convergence zone, for example, may fluctuate in position or intensity, but it, and its contributory airflows, will rarely, if ever, be absent. Nevertheless a single mean, even a vector mean, is apt to be a dangerous tool if not treated carefully. My chief reason however for rejecting the mean was the colossal amount of labour which would have been involved. It should be realized that this paper was prepared for an area for which many of the records, none too voluminous in any case, had been destroyed by an enemy or occupying power. A complete rebuilding of the meteorological establishments had to go hand in hand with an unprecedented and overwhelming demand for day-to-day services, chiefly from aviation. The old climatological ideas, it was soon obvious, did not apply. Against this urgency, and with no time, personnel, or even material in many cases from which to calculate means, this paper was written, and the obvious mode adopted.

Like Dr. Crowe I had difficulty with the 2,000 ft winds over the Bay of Bengal in May. It might have been better had I made my 'transition' season April and May. The Indian Meteorological Department however, in the publication I quoted, gives the normal date of onset of the SW. Monsoon on 25 May as lying from Ceylon to near Rangoon, despite predominant SW. winds in May over the Bay of Bengal at 1,600 and 3,300 ft shown in the upper-winds section. Herein lies much of the danger in a simple mean wind. There is much fluctuation in May which is a difficult month to treat with a single-model chart, as I implied in my first section. I find no difficulty in explaining the earlier formation of the Indo-China thermal low than the Indian. South of 20°N, Indo-China has an area probably larger than that of India. Moreover its topography is different – there is a large central very low-lying plain bounded east, west and north by mountains, narrow to east and west. Such a basin is eminently more suitable for the development of an early low than the plateau country of southern India.

I would remind Mr. Bonacina that new ideas do not normally or necessarily correlate with older, and note that the SW. monsoon of India, with its 'copious supplies of vapour,' at a slightly earlier stage in its history provides the dry season of Kenya and Tanganyika, even

though the forced ascent over the latter territories is often greater than that over India. Moreover such rain as does occur over the Kenya Highlands during this season is chiefly associated with air from the *west*, not from east or south-east. These facts, not to my knowledge heretofore sufficiently emphasized, should give food for thought to any who consider the monsoonal rains of India as essentially simple of explanation. It is also noteworthy that a front (a density discontinuity) is, normally, a ' convergence,' but as a tropical meteorologist I am not yet prepared to accept that a ' convergence ' is necessarily a front.

I am indebted to the Director, East African Meteorological Department, for permission to reproduce the two charts.

The dissipation of kinetic energy in the lowest layers of the atmosphere

By R. J. TAYLOR

(Read 18 June 1952 (presented by Dr. F. Pasquill). See *Q.J.*, **78**, p. 179)

Asst. Prof. P. A. Sheppard : This paper is specially to be welcomed as a promise of the analyses which we may hope increasingly to obtain on the role of small-scale turbulence in the fundamental dynamics and thermodynamics of the atmosphere.

The author's conclusion that advection of turbulent energy from one level to another is negligible in the few cases examined is one we must certainly not generalize without further investigation. Thus in the one case cited in Table I for the level of 29 m (Run 5) the advective flux begins to merit consideration with the other terms in the energy balance. While unlikely ever to be dominant in this bottom layer it may yet play an important role in higher levels.

Some readers will probably have been taken by surprise at the large heating implied by the author's rates of dissipation of kinetic energy. Thus the mean dissipation over the bottom 2 m in Runs 1 and 2 corresponds to a temperature rise of 3·3°C/hr in the layer ! Moreover, as the author points out and as we may agree, the rate of heating is roughly proportional to the cube of the velocity. Dr. Pasquill's reminder that we may not neglect such a factor in the thermal economy of the lower atmosphere is therefore very timely.

Finally may I draw the author's attention to some confusion on p. 180 where the flux F as defined by the first equation is an upward flux but is described (first two lines) as a downward flux. Since F proves to be negative ($\simeq -\tau u_h$ in which τ and u_h are positive) the actual flux *is* of course downward. There is also an error of σ for δ in the second term on the right of the last equation on the same page.

Dr. G. D. Robinson : Mr. Taylor speaks of the importance of the dissipation of energy by turbulence in the heat economy of the lowest layers of air. It is possible to exaggerate this importance. I will use as illustration some figures obtained by Mr. A. J. Lander and myself at Kew Observatory, using Swinbank's method. The apparatus was small (about 1 cm cube overall), responded to fluctuations of period 0·1 sec, and recorded over 1-min intervals. It was set 25 cm above a surface of very short grass. Two examples are shown in Table I, taken on 25 October 1950 shortly after sunset. There was no cloud, and the average wind speed at 1 m was about 300 cm sec^{-1}.

TABLE I. Vertical flux of various quantities through the 25 cm level (positive upward)

Quantity	Example 1 (erg cm^{-2} sec^{-1})	Example 2 (erg cm^{-2} sec^{-1})
Total kinetic energy	− 18	− 180
Turbulent kinetic energy	+ 2	+ 4
Heat	$-2{\cdot}3 \times 10^4$	$-4{\cdot}1 \times 10^4$

It is clear from the table that the kinetic energy fluxes are very small in comparison with the heat flux and a consideration of the radiative heat transfer in the layer (see e.g., Robinson, *Q.J.*, **76**, p. 37, Sec. 6) shows that the divergence of the vertical convective heat flux is normally very much larger than temperature changes would suggest, being of the order of 10 per cent in the lowest metre. The heat produced by dissipation of kinetic energy in the layer has therefore not normally the relative importance which Mr. Taylor suggests for it, though it may have occasionally.

Another feature of the Kew results may be relevant. It is found that persistent mean vertical velocities exist at a point quite close to the ground, e.g., 5 cm sec^{-1} at 25 cm height and 20 cm sec^{-1} at 150 cm height. In these circumstances the term $\bar{u}\,\bar{w}$ or $\bar{w}\,\bar{T}$ may well be 100 times greater than the term $\overline{u'w'}$ or $\overline{w'T'}$ at the point of observation. The vertical velocities are probably due to undulations of the ground and to standing eddies caused by obstacles or vegetation, and may well be associated with vertical transport of kinetic energy. (The Kew observations suggest but have not yet definitely established that they are responsible for a considerable part of the heat transport.)

Mr. Taylor's conclusion that the upward transport of turbulent kinetic energy is negligible is novel and important, and has some consequences which are not easily explained. It would be reassuring to have it confirmed by an integration of the transport over an area, as well as in time, but the task is formidable.

Dr. F. Pasquill (*in reply*) : On the subject of persistent vertical velocities I might mention an interesting point which has emerged in recent work at Harwell. In a study of the scattering of smoke puffs emitted at 100 ft or so above ground, it was found that in unstable conditions vertical currents in one direction or the other were sometimes sustained at a point in space for periods of several minutes.

In general reply to preceding contributions, there is, of course, no suggestion in Mr. Taylor's paper that the results are to be accepted as general. Indeed it is clear at the outset that consideration is being restricted to occasions in which the departures from effectively neutral conditions of stability are small. I think the point to be emphasized is that the paper provides the first serious attempt to deal quantitatively with a term in the energy balance which is generally expected to be small. This demonstration of the small magnitude of the flux of kinetic energy term is comforting to those who have previously neglected it in conventional heat-balance considerations. As to future work, I think that a most valuable development would be the extension of the analysis to markedly stable and unstable conditions where damping and growth of turbulent activity are involved.

Mr. R. J. Taylor (*in reply, communicated*) : I have no hesitation in agreeing with Prof. Sheppard's remarks on the impossibility of drawing any general conclusion that the diffusive flux is negligible. I must draw his attention to what appears to be an arithmetical error in his figure of 3·3°C/hr in the lowest two metres for Runs 1 and 2. According to my calculation, this should be reduced by approximately one-half.

It is clear from Dr. Robinson's remarks that he relies on the existence of a comparatively large divergence in the radiant heat flux to infer the existence of an opposite divergence of similar magnitude in the turbulent heat flux. Care should be taken, however, in extending his arguments. It was shown in my paper and emphasized by Professor Sheppard that dissipative heating accounted for a considerable part of observed rates of rise of temperature, leaving only a fraction of this rate of rise to be accounted for in terms of the divergences of radiant and turbulent heat fluxes. When we consider the energy balance of a layer of air, we should attach importance to the sum of these divergences rather than to their individual magnitudes. The mean vertical velocities mentioned by Dr. Robinson are, as he says, no doubt local in origin. It might well be expected that they are the less important the more open the exposure of the site. It would, of course, be most unwise to consider as representative of an area a measurement of eddy flux made while a large mean vertical velocity existed.

I should like to take the opportunity of thanking Dr. Pasquill for having undertaken the presentation of this paper on my behalf.

Structure of the upper westerlies; a study of the wind field in the eastern Atlantic and western Europe in September 1950

By R. MURRAY and D. H. JOHNSON

(Read 18 June 1952. See *Q.J.*, **78**, p. 186)

Mr. E. Gold : I have recently been looking at the records of upper wind at Larkhill and Fazakerly, more particularly at the 800-mb level. In the four years 1948-51, September, like May, is usually a relatively quiet month, with no winds at the 800-mb level stronger than 50 kt. But Sept. 1950 was exceptional and had more observations of winds stronger than 50 kt, at Fazakerly, than any other month of that year. As the month was exceptional in this respect, is it possible that its jet streams were also exceptional, i.e., non-typical of the general run of jet streams ?

Mr. J. L. Galloway : I have often thought that it is noteworthy that in the now familiar cross-sections of jet streams as shown in Figs. 2 and 3 the isotachs are always drawn as being continuous across the tropopause. The existence of a wind field in the stratosphere indicates the existence there of a close horizontal temperature field : I think that this is a point that is often overlooked owing to the emphasis placed on the absence of lapse in the stratosphere. Nevertheless, it has often seemed to me, on inspection of tephigrams, that for comparable wind shear the temperature gradient is less in the stratosphere than in the troposphere. I wonder if the thermal wind equation holds without modification in the stratosphere.

Mr. J. K. Bannon : In the neighbourhood of the British Isles temperature contrasts are often greater in the winter than in September. Has Mr. Murray noticed any important differences between winter jet streams and those investigated for Sept. 1950 ?

There is a semi-permanent jet stream across the south of the United States in winter. American meteorologists have stated that the horizontal shear to the south of this jet is of the same or greater magnitude as the shear on the polar side. Mr. Murray has shown that the reverse is true of jet streams near the British Isles. Can he offer any explanation ?

Asst. Prof. P. A. Sheppard : This paper is surely an admirable descriptive account of the *horizontal* motions of the jet-stream complex and to a less extent of the associated field of temperature. But a complete description must of course include the associated field of vertical motion and I would ask the authors whether they see their way forward to this. In particular is there any evidence, from the case histories of the jets of Sept. 1950, for inferring the role of the vertical motion in transforming the field of temperature and with it, the horizontal wind field ?

Dr. R. C. Sutcliffe : Mr. Murray and Mr. Johnson have, I think, made a valuable contribution to the literature on upper-wind structure. Although they present no fundamental discoveries and the general results may be familiar to many synoptic meteorologists who have worked in this field it is most important that, in order to obtain a balanced view on the structure of the atmosphere, we should have not only text-book models and studies of selected special cases but also an impression of the many varieties which arise in the real complexity of nature. The position is analogous to that with frontal analysis : the classical models are excellent material for initial courses of instruction and also as a basis for theoretical formulation but one knows little about fronts who has not studied their great variety in day-to-day synoptic work. The present paper will therefore fill a real gap in the literature, especially for our part of the world.

There is still quite a lot we do not know about the upper-air synoptic climatology of the world. I do not think that we are yet quite certain about the relationship between the highly fluctuating jet streams associated with the ' unsettled westerlies ' and the high-level sub-tropical jets which are regarded apparently, and perhaps correctly, as girding the earth. To put a girdle round the earth is a worthy human ambition but there are wide areas of the tropical and sub-tropical oceans about which we have very little day-to-day synoptic knowledge from the upper air and we must be careful not to stretch the loose ends of the continental jet streams too confidently across the oceans.

Mr. R. Murray and Mr. D. H. Johnson (*in reply*) : Mr. Gold is quite right in saying that Sept. 1950 was much more unsettled than normal over the British Isles, but evidence from other work suggests that the September jet streams were not exceptional. During Sept. 1950 the main thermal gradient and mean jet stream were over the British Isles : in a normal rather quiet September over the British Isles the jet streams tend to be located farther north, perhaps between Scotland and Iceland.

In reply to Mr. Galloway, the thermal gradient in the stratosphere is well-known to be in the reverse direction to that in the troposphere. Above jet streams the stratospheric thermal gradient is usually strong although variable from case to case. On some occasions over a limited depth of perhaps 5,000 ft the thermal gradient may be greater than anywhere in the troposphere. We have not noticed that the thermal wind equation, which is a geostrophic relationship, is less applicable to the stratosphere than to the troposphere, nor do we see any theoretical reason for it to be so.

As Mr. Bannon has mentioned, the temperature gradients are on average greater in winter than in early autumn, but we have not observed any real difference in the characteristics of jet streams according to the season. Of course the strongest wind maxima and extreme values of horizontal and vertical shears almost certainly occur in the winter, but such differences are of degree only. Many jet streams apart from those of Sept. 1950 have been examined and have shown stronger shears on the low-pressure side than on the high-pressure side. The winter jet stream over southern U.S.A. appears to have stronger horizontal shears to the south than to the north of the axis, but this may characterize low-latitude jet streams if there is a tendency for a subsidiary jet stream to occur in somewhat higher latitudes. There is however some doubt about the reality of the phenomenon as most of the measurements of shear south of the American semi-permanent jet stream are geostrophic and, as is well-known, the geostrophic approximation becomes rather poor in very low latitudes.

As Professor Sheppard has pointed out, it is most desirable to describe the field of vertical motion as well as the horizontal fields of wind and temperature, so as to get some insight into the dynamical and thermodynamical inter-relationships associated with jet streams, but this is clearly a difficult problem. Our aim in writing the paper was less ambitious. Some other work with which one of us has been associated at Dunstable does however suggest the existence of a ' direct ' transverse vertical circulation at the entrance and an ' indirect ' circulation at the exit to jet streams, although of course this may not be the whole story.

Proceedings at meetings of the Society

Ordinary Meeting, 18 *June* 1952

The following candidates were declared to have been elected that day by the Council :

Fellows

Birkett, John Allan, Lakeham, Ashton, nr. Exeter.
Cherry, Stanley Edward, F.R.G.S., ' Teign Croft,' 87 Spenser Road, Bedford.
Clark, Douglas Jim, 13 Clitheroe Avenue, Harrow, Middlesex.
Davies, Trevor Mills, B.Sc., 10 Park End Road, Gloucester.
Goodall, Henry James, Yew Tree Cottage, Bent Lane, Whitmore, Newcastle-under-Lyme, Staffs.
Pereira, Gerard Ian, Air Service Training Ltd., Hamble, Hants.
Preston, William, Ashley Cottage, Seaton Ross, York.
Ross, George Bain, M.A., Longnewton Mill House, Melrose, Roxburgh, Scotland.
Scott, John Robert, B.Sc., c/o Meteorological Office, P.O. Box 715, Singapore.

Foreign Members

Bodurtha, Lt. Frank Theodore, U.S.N.R., Russellville Road, Southampton, Mass., U.S.A.
Franco, Pedro Rodriguez, Modesto Lafuente No. 49-1^0, Madrid, Spain.
Jacobs, Major Robert A., B.A., Walnut Tree Cottage, Aynho, nr. Banbury, Oxon.
Markham, Capt. Charles G., B.A., 11 St. Patrick Street, Rapid City, South Dakota, U.S.A.

Associate

Johnston, Ewart Albert, 34 Fernbank Avenue, Toronto 4, Ontario, Canada.

Student Member

Houghton, John Theodore, B.A., Jesus College, Oxford.

The following papers were read and discussed :

(*a*) ' The dissipation of kinetic energy in the lowest layers of the atmosphere ' by R. J. Taylor (*Q.J.*, **78**, p. 179). The paper was presented by Dr. F. Pasquill.

(*b*) ' Structure of the upper westerlies : a study of the wind field in the eastern Atlantic and western Europe in September 1950 ' by R. Murray and D. H. Johnson (*Q.J.*, **78**, p. 186).

Summer Meeting, 16 *July* 1952

The Summer Meeting of the Society was held on 16 July 1952 at London Airport, by courtesy of the Commandant, Air Marshal Sir John D'Albiac. Some 80 Members and guests attended and enjoyed a very interesting and instructive afternoon, visiting the Meteorological Office, Air Traffic Control, Flight Briefing, and making a tour of the airport. The party was also conducted over a Hermes aircraft of the B.O.A.C. A full account of the meeting was published in the August number of *Weather*.

Report on the Snow Survey of Great Britain for the season 1949-50. By E. L. Hawke and D. L. Champion. Reprinted from the *Journal of Glaciology*, London, **1**, No. 9, 1951. Pp. 11. Price 2/6. (1950-51 : *ibidem*, **2**, No. 11. Pp. 15. N.p. 1952).

The publication by the British Glaciological Society of the results of its Snow Survey of Great Britain has become an annual event; and the appearance of a further summary for 1950-51 should be noted, as it gives some data with regard to a winter characterized by exceptional persistence of snow on our northern uplands. The survey was recommenced with energy under its present directors in 1946-7. With the aid of a large number of volunteer observers, in addition to the data provided by the Meteorological Office, it has evolved a long way beyond that initiated by this reviewer in 1938 which had of necessity to be brought to an end early in the war owing to pressure of other work. Extensive descriptive summaries of the occurrence of snowfall have now been provided for several years; and, knowing something of the work involved, all who read the reports will agree with the present writer in paying tribute to the immense amount of voluntary work thus undertaken.

Nevertheless it is the opinion of this reviewer that while no harm is done by observing the incidence of snow, the Survey is accumulating far more lowland observations than are now necessary for the furtherance of knowledge. Little or no reference appears to be made to the very extensive accumulation of published observations provided by the Meteorological Office since 1912. Since the reviewer had reason to comment (*Q.J.*, **65**, pp. 12-14, 1939) on the need for further information in regard to certain upland districts, for example in the Midlands, a large number of well-staffed airfields has been established at which first-class observations of snow, sleet and snow-cover are now maintained. In the light of knowledge already gained, our official stations now provide an adequate framework of data up to the 1,000 ft level for practically every part of Britain with the exception, perhaps, of central Sutherlandshire, and it may be doubted whether much is now gained by adding so extensively to the official summaries in the *Monthly Weather Report*. Moreover, with regard to the summaries of data from places adjacent to the coast, it may also be questioned whether the assemblage of large quantities of observations of the frequency of snow and sleet is going to lead any further beyond what we already know, namely that under British climatic conditions the comparative alertness of observers must always be reckoned with when questions arise of the occurrence of a few flakes of sleet amid the rain. After all, not many years ago there were two neighbouring stations in the London area at one of which, 250 ft above the other, the number of days with snow or sleet for several years averaged little more than one-third of that reported by the lower station. Such discrepancies are fewer than they were; but they exist.

But the upland observations, referring to those levels on our mountains above 1,500 ft (which incidentally is very nearly the limit of permanent human settlement in Britain) undoubtedly possess potential value. The tabulated durations of mountain snow-cover at various levels are pleasantly depicted by Mr. Champion although, to be strictly accurate, the 3,000-ft level is not visible from Tairbull, and the 3,500-ft level is not visible from Glen Brittle or on Ben More (Mull). His more generalized graphs showing the rate of increase of the annual number of days of snow-cover with altitude will no doubt confirm what we already know, that over a longer term of years the increase on any given range in any given aspect is virtually linear. The data provided in these reports must not be used uncritically, for any mountaineer is aware of the importance of aspect and this is not made sufficiently apparent in the text or otherwise. For example we read in the 1950-51 report that over the last five seasons ' it would seem that the average duration of snow-cover on the Brecon Beacons down to about 2,000 ft in February exceeds that at similar levels on Bowfell (Lake District).' Scrutiny soon reveals that in the first case the steep northerly crags are being observed, while Bowfell is seen from the south-east.

That these upland data are useful is however unquestionable; in time, no doubt, the results can be integrated with our present knowledge. The report, for example, of 230 days with snow-cover (1950-51) on top of Ben Nevis is quite satisfactorily accordant with the Observatory records during 1883-1904 and with the experience of numerous observant mountaineers since then.

The collection and compilation of such a vast bulk of observational data indeed testifies to an abundance of keenness and energy in regard to an interesting aspect of British climatology. In the reviewer's opinion, however, more attention might be paid to what may be called the design of the experiment; to the purpose with which such observations are to be collected; and to the extensive work which has already been done. After all, observations of mountain snow-cover were made in Scotland nearly a hundred years ago; numerous foundations exist on which to build. Our mountain snows have been the subject of comment since Taylor saw them in the Cairngorms in 1618 and Burt in 1727; the variations in their duration and incidence are worthy of regard on account of Britain's past climatic history on the margin of glaciation, as well as in connexion with the behaviour of our mountain flora and fauna. For these reasons, the Survey will fulfil a useful function and it is to be hoped that its upland activities will continue; a valuable beginning has been made.

G.M.

Heat and thermodynamics. By M. W. Zemansky. London (McGraw-Hill). 3rd Edition, 1952. Pp. XIV, 465; Figs. 42s. 6d.

This book is more restricted in scope than most of the many others which bear the same title. In particular all reference to kinetic theory and statistical mechanics is omitted in order, in the author's words, ' that the power and limitations of the macroscopic point of view may be clearly understood, and not confused with those of the microscopic point of view.' The author's object is to give students of about University-entrance standard a sound introduction to the fundamental concepts of thermodynamics, and an indication of the fields of application of thermodynamic methods, particularly in physics and engineering. He has written a teaching book rather than a reference book – there are practically no citations of original work, only of other text books – and it is hard to see how his work could have been better done. The very high reputation for soundness and lucidity which was achieved by the earlier editions has not prevented the author from making extensive revisions in detail in all chapters, and new chapters on heat transfer and on the physics of very low temperatures have been added.

The problems which follow each chapter are a very valuable feature. They are chosen to test the student's grasp of principles rather than to introduce him to likely examination questions, as may be seen from an example of some meteorological interest – ' In the tropics the water near the surface of the ocean is warmer than the deep water. Would an engine operating between these two levels violate the second law ? '

G.D.R.

A treatise on heat. By M. N. Saha and B. N. Srivastava. Allahabad (The Indian Press). 3rd edition. 1950. 8vo. Pp. XII, 935. 3 plates, figures. Rs. 32.

Heat, the most fascinating of the conventional major divisions of physics, is probably the best served with text books; this treatise is the third edition of an established favourite noted for the wide range of topics it embraces. It was first published in 1931 and the second edition followed quickly in 1935. The present edition is enlarged and has been extensively rewritten and rearranged; it is effectively a new book. Thermometry and calorimetry are discussed in about 100 pages, and expositions of the fundamentals of the kinetic theory and of thermodynamics are each given rather more space than this. A chapter on classical statistical mechanics follows. Some 300 pages are then devoted to the use of these methods in explaining the thermal properties of matter – equations of state, change of phase, thermal expansion and the transmission of heat. The final chapters, covering quantum statistics and the thermodynamics of chemical equilibria, are particularly noteworthy for an exposition of the authors' work on thermal ionization. The final 100 pages is an Appendix of 19 notes, ranging inconsequentially from ' Proof of Liouville's theorem ' to ' Occurrence of the elements and their isotopes on the earth, the meteorites, the sun and the stars.' There are numerous references to the original work of American, Asiatic, and European physicists – many western authors convey the impression that their subject is a national monopoly – and a wide range of applications of the various principles are discussed in the text. Meteorologists, for example, will note paragraphs on conduction of heat in the soil and on cloud physics, including a reference to ' rain-making.' The treatise can be strongly recommended both as a source book and a text book for the honours student.

The actual production of the book is much less admirable. There is something to be said for the use of poorish-quality paper in a book as ephemeral as an up-to-date scientific text, particularly if the price is thereby kept in bounds, and the duplication of pp. 665 to 672 at the expense of pp. 673 to 680 is no doubt a peculiarity of the review copy, but the numerous minor misprints and misalignments and the capricious use of capitals imply somnolent proof reading and sub-editing. There is more excuse for the occasional lapses in revision which amongst other things allow the word ' recently ' to mean any time from 1933 onwards. These are minor flaws and should deter no one from buying the book : there is little doubt that this edition will repeat the great success of earlier ones.

G.D.R.

Geographica, No. 22. *Climatological studies in Uppsala with special regard to the temperature conditions in the urban area.* By Ake Sundborg. Uppsala (Uppsala Universitet Geografiska Institutionen). 1951. Pp. 111; 35 figures, 10 tables. 9 kr.

This very informative volume deals with a problem which is readily accepted but rarely investigated in detail, namely, the difference between urban and rural climates. It is described as a micro-climate investigation but it is thought that local climate or even meso-climate would be a better description. Two points are of especial interest; one is the choice of rapid temperature surveys by resistance thermometers mounted on a motor car as a method of assessment of climatic differences in various types of weather throughout the year; the other is the detailed attention given to differences in radiation exchange as the basic factor. Several excellent isopleth diagrams and cross-sections (or profiles) are given, chiefly

of temperature, and other elements such as humidity and wind velocity are considered. The report also contains a brief summary of the literature on urban climatology and a 3-page list of references. There is an excellent chapter on the general survey of the Uppsala district and an attempt to relate the town – country temperature contrasts with simple climatological elements. The translation, diagrams and general presentation are of a very high standard, and there are few occasions when the line of argument is not clear. The whole may be fairly regarded as a pattern for investigations of this nature and the results obtained are of prime importance in the interpretation of ' standard ' climate.

L.P.S.

Atti del Convegno Internazionale di meteorologia marittima Genova 20/22-ix-1951. Milan (Instituto Geofisico Italiano). 8vo. Pp. iv, 181; 7 plates, many figures and tables. N.p.

Of the 22 papers presented at this International Conference 12 are printed in Italian, 5 in German, 3 in French and 2 in English.

One's general impression is that the contributions are of uneven quality; some papers discuss subjects of far-reaching importance while others are of local interest only.

Of particular interest to the present reviewer is a paper in which H. U. Roll discusses his and other work on airflow over the sea. The broad connection between his results and those presented in a short but important paper by A. H. Gordon poses a major problem.

H.C.

Waldgeschichte Mitteleuropas. By Franz Firbas. Erster Band : *Allgemeine Waldgeschichte.* Jena (Gustav Fischer). 1949. 8vo. Pp. 480; maps, diagrams. DM 24. Zweiter Band : *Waldgeschichte der einzelnen Landschaften.* 1952. 8vo. Pp. 256, figures. DM. 12.

The full title of this typically thorough work, *Spät-und nacheiszeitliche Waldgeschichte Mitteleuropas*, gives a better indication of its scope; undoubtedly it will be much appreciated, coming as it does from one of the best known and most widely-cited authorities on forest history, who holds the Chair of Botany at Göttingen. German work in this field has been abundant, as is very proper to those who dwell in the great Hercynian forests : Firbas provides a quite phenomenal set of references, not only to the studies of his own countrymen, but also to a wide range of authors from the adjacent lands lying north of the Alps, from Ireland to Finland.

In the nature of things such a work falls outside the normal range of the meteorologist; and in turn its botanical content is somewhat specialized. But it will provide an indispensable compilation of material for students of past climatic fluctuation in Central Europe so far as the trees can provide the evidence. In this connexion meteorologists will recall Faegri's reminder of the need for care in interpretation (*Cent. Proc. R. Met. Soc.*, 1950). Firbas adopts a characteristic German approach; in the first section he reviews all the methods open to those who wish to study forest history. In the second section of his first volume he gives a very fine review of the relationship between forest development, glacial retreat and subsequent climatic fluctuation; in later sections he goes on to consider in greater detail the spread of particular species; and later still, the relationship between the spread of settlement and forest development down to the Middle Ages. References are brought down to the date of publication, and include at least one from *Weather*. In the second volume the forest history of Germany is treated in equal detail on a regional basis. The whole work integrates the results of the very widespread pollen – analytical and other studies of forest history in a most impressive way. Diagrams and maps abound, rather small in size at times. Readers will be very ready to congratulate the author on the exemplary way in which, in spite of the numerous post-war difficulties, he has brought his sixteen years' labour to fruition and provided an indispensable work of reference. The author is not given to speculation; instead, he provides a masterly compilation of the evidence concerning what trees were present and where, throughout north-central Europe, since the Ice Age; which is to be strongly commended to all interested readers of this Journal.

G.M.

Old English Barometers. By G. H. and E. F. Bell. Winchester (The Wykeham Press). 1952. Crown 4to. Pp. IX, 42; 40 plates. 47s. 6d.

Outside the field of horology the barometer is unique among scientific instruments in having established itself as a decorative piece of domestic equipment. The history of the barometer is thus of interest from two widely different aspects, as an instrument of precision for the use of meteorologists and physicists and as a ' weather glass ' for domestic use. The scientist expects his barometer to be well designed, accurate and soundly constructed. Any beauty of appearance it may possess is purely incidental. In the domestic field however appearance is of over-riding importance. The barometer must be designed to harmonize with schemes of furnishing and decoration in much the same sort of way as a clock does.

In 1886 Mr. William Ellis of Greenwich Observatory, then President of the Society, wrote an xcellently documented ' Brief historical account of the barometer ' which was published in the *Quarterly ournal*. It is surprising however that nobody has ever written a comprehensive treatise on the barometer s a scientific instrument. The book by G. H. and E. F. Bell does not attempt to cover this field, the mphasis being almost entirely on the development of the domestic barometer as a decorative object. Nevertheless this is a book which a meteorologist may dip into with a great deal of pleasure. Lavishly llustrated, it covers the period from Torricelli to the invention of the aneroid, that is to say roughly rom the middle of the seventeenth to the middle of the nineteenth century. The earliest barometers llustrated date from the late seventeenth century and the latest is FitzRoy's of 1860.

The earliest makers of domestic barometers were well-known clockmakers such as John Smith, Henry Jones, Daniel Quare and Thomas Tompion. These artist-craftsmen were already expert at giving heir products the handsome external appearance demanded by their patrons. It is not surprising therefore hat these early makers, and particularly Quare and Tompion, produced some of the most beautiful barometers ever made.

But even the best designers could not cope with the ungainliness of the ' diagonal ' barometer in which the upper part of the stem was bent nearly at right angles and greatly elongated so as to obtain a large movement of the mercury meniscus for a small change of pressure. One feels that the authors have devoted too much space to these and other monstrosities, while barely mentioning a major improvement like the vernier which is figured in an early nineteenth century barometer by Smith of Royal Exchange.

Readers who may be tempted by this fascinating book to become collectors of ' vintage ' barometers will find encouragement in a remark (p. 17) that ' many are still to be purchased at reasonable prices.'

E.G.B.

Recent additions to the Society's Library

Archaeology and natural resources of Natal. Capetown, University of Natal, 1951. Pp. 140. N.p.

G. H. and E. F. Bell, *Old English Barometers.* Winchester, Wykeham Press, 1952. Pp. ix, 42. 47s. 6d.

N. Bjerrum, *Structure and properties of ice.* Copenhagen, E. Munksgaard, 1951. Pp. 56. 5 kr.

Co-operative snow investigations. U.S. Weather Bureau.

(*a*) *Hydrometeorological log of the Willamette Basin Snow Laboratory.* 1947-48 *and* 1948-49 *Water Years.* U.S. Army, 1951. Pp. 107. N.p.

(*b*) *Hydrometeorological log of the Central Sierra Snow-Laboratory* 1948-1949 *Water Year.* Pp. 268,24. N.p.

(*c*) *Technical report No.* 24. *Hydrometeorological log of the Upper Columbia Snow Laboratory* 1948-1949 *Water Year.* 1952. Pp. 372, 70. $4.75.

(*d*) 1949-50 *Water Year.* 1952. Pp. 255, 6. $3.50. 4 parts.

F. Firbas, *Spät-und nacheiszeitliche Waldgeschichte Mitteleuropas nördlich der Alpen.* Vol. 2. Jena, Gustav Fischer, 1952. Pp. 256. 12 DM.

E. L. Hawke and D. L. Champion, *Report on the Snow Survey of Great Britain for the season* 1949-50. London, *Journal of Glaciology,* 1951. Pp. 11. 2s. 6d. *Ibid.,* 1950-51. Pp. 15. N.p.

S. P. Jackson, *Climates of Southern Africa.* Johannesburg, *South African Geographical Journal,* 1951. Pp. 21. N.p.

Meteorological Office, *Upper air data for stations maintained by the Meteorological Office,* 1946-50. *Part* 1. *Larkhill.* London, H.M.S.O., 1952. Pp. 52. N.p.

Meteorological Service, Argentine, *Climatological and geomagnetic data for Oreadas Islands* 1903-50. Buenos Aires, Argentine Meteorological Service, 1951. Pp. 97. N.p.

Meteorological Service, Belgian Congo, *Bulletin du Service Météorologique du Congo Belge.* Elizabethville, 1951. Pp. 98. N.p.

Report of the Astronomer Royal to the Board of Visitors of the Royal Greenwich Observatory. Herstmonceux, Royal Greenwich Observatory, 1952. Pp. 30. N.p.

J. F. Scott, *The scientific work of René Descartes.* London, Taylor and Francis, 1952. Pp. 211. 20s.

H. C. Urey, *The Planets.* London, Geoffrey Cumberlege, 1952. Pp. 245. 30s.

R. Von Mises and T. Von Kàrmàn, *Advances in applied mechanics,* Vol. II. New York Academic Press, 1951. Pp. 233. $6.50.

World list of scientific periodicals published in the years 1900-1950. W. A. Smith and F. L. Kent, Eds. 3rd edition. London, Butterworths, 1952. Pp. 1058. £10. 10s.

Presented to the Library by Mr. D. J. Schove :

E. Vanderlinden, *Chronique des événements météorologiques en Belgique jusqu'en* 1834. Brussels, Belgian Royal Academy, 1924. Pp. 329. N.p.

Obituary Notices

Dr. A. CRICHTON MITCHELL, F.R.S.E.

By the death in Edinburgh on 15 April 1952 of Alexander Crichton Mitchell this Society lost its Senior Fellow and a former honorary secretary (1928-1933) and vice-president (1934-1935). His election to life membership of the Scottish Meteorological Society (amalgamated with the Royal Meteorological Society in 1921) took place as long ago as 1891. Crichton Mitchell served at one time as Director of Instruction at Travancore, India. In 1916 he was placed in charge of Eskdalemuir Observatory and in 1922 became the first superintendent of the then newly-established Meteorological Office headquarters in Edinburgh. Although he published several papers on the diurnal variation of temperature and pressure and played an important part in the reorganization of meteorological work in Scotland, he was primarily a geophysicist. It was for his studies of terrestrial magnetism that he was best known. To this he devoted an immense amount of painstaking research, which included latterly a valuable review of the subject in its historical aspect. Seismology was among his principal subsidiary interests. In collaboration with J. J. Shaw he contributed to the *Dictionary of applied physics* the article on seismometry.

Much of Crichton Mitchell's time was given in the service of the Royal Society of Edinburgh, to the company of whose Fellows he was admitted in 1889. For many years he was curator of the library, and from 1926 to 1929 he held the office of vice-president. That Society's 1931-33 Keith Prize was awarded to him for his work on the diurnal incidence of disturbance in the terrestrial magnetic field. From 1928 to 1937 he was a member of the Advisory Committee on Meteorology for Scotland.

Possessed of tireless energy and unbounded powers of concentration himself, Crichton Mitchell could not brook slackness or lack of enthusiasm in his associates. To him work was the *raison d'être* of life and so, he felt, it should be to others. On one occasion, some 25 years ago, he invited a young Scottish scientist who was about to enter the service of the Meteorological Office at Edinburgh to visit him for a discussion of the forthcoming appointment and its prospects. The invitation was for breakfast time – 8-15 a.m. On arrival the guest ventured to express surprise at finding his host hard at work so early in the day. 'I've been at it for four hours,' boomed the senior man in his deep voice. Crichton Mitchell was given to plain speaking and seldom minced his words in any company. These qualities – his bluntness and his intolerance of slovenliness or inefficiency – won him the respect of all with whom he had to deal, though naturally not always their affection.

Dr. Crichton Mitchell retired from his official duties in the government meteorological service on reaching the age of 60 in 1925. But with him retirement did not mean leisure. For another 27 years he laboured on, seeming to need little relaxation even in advanced old age. Until a short while before his death he was still busily planning further lines of research.

E.L.H.

Major J. S. ELLIS, J.P.

Major John Shipley Ellis, who died on 25 February 1951 at the age of 86, was a director and chairman of Messrs. Ellis and Everard, Ltd., coal and agricultural merchants operating in the east midland counties of England. He had been interested in meteorology since his boyhood and maintained a continuous record of rainfall at his Peterborough home from 1907 until the onset of blindness intervened not long before his death. Major Ellis was elected a Fellow of this Society in 1918.

E.L.H.

Dr. R. E. WATSON, Ph.D., B.Sc.

Robert Edward Watson, who died suddenly on 28 May 1952, was born on 1 April 1889. After studying at University College, Nottingham and taking the B.Sc. degree of London University in 1911 he became a lecturer in mathematics and physics at Halifax Technical College. He entered the Meteorological Office in 1918, having served in the artillery during the 1914-18 war, and having been twice wounded and invalided out of the Army.

The first sixteen years of Watson's career in the Meteorological Office were devoted to the work of the observatories. This part of his career began by his being appointed as officer-in-charge at Falmouth Observatory soon after joining the Office. He was a member of the staff of Kew Observatory from 1920 to 1929 and was Superintendent of Eskdalemuir Observatory from 1929 to 1934. He worked with Dr. Chree for the first five of his nine years at Kew and subsequently with Dr. Whipple. He came to share their keen interest in geophysics, and the period at Kew was most fruitful scientifically. His papers on geophysical subjects included two in collaboration with Dr. Chree published in the *Proceedings of the Royal Society* and three (one in collaboration with Dr. Chree) published by the Meteorological Office in *Geophysical Memoirs*. The subjects embrace atmospheric pollution related to potential gradient, comparison between underground and surface magnetic records, pyrheliometer comparisons and electrical conductivity of the air related to the earth's electric field near the ground. Watson was awarded the degree of Ph.D. by London University in 1927. From 1934 onwards he was concerned with the application of meteorology to aviation and was as successful in his work with the Royal Air Force as he had been

earlier in the more recluse life of an observatory. In the early war years he surveyed war-tim requirements for the meteorological training of air-crew and later in the war did distinguished wo as senior meteorological officer at a Group Headquarters in Bomber Command with control of a numb of subsidiary meteorological offices. He took personal responsibility for much of the forecasting f bomber operations carried out by his Group. In 1948 he was appointed chief meteorological office Bomber Command, a post he held until he retired in March 1951, having served the Meteorological Offi for more than 32 years. He was elected a Fellow of the Society in 1921.

Dr. Watson took a broad view of life. He was fond of sport, of good companionship, and was himse a witty conversationalist. The several facets of his character endeared him to a wide circle. He will b missed by many colleagues in the Meteorological Office and by many friends in the Royal Air Forc

M.T.S.

The Reverend H. W. C. GELDART, M.A.

The Reverend Henry Walter Cabell Geldart, a Fellow of the Society since 1930, died in February 195 at the age of 88. He received his education and training at Emmanuel College and Ridley Hall, Cambridg and at the Church Missionary Society College, Islington. For a time he was principal of the Isle of Ma Theological School and chaplain to the Bishop of Sodor and Man. Later he held livings in Derbyshir Suffolk and Hampshire. Retiring in 1937, he took up residence at Hastings, his native town.

E.L.H.

Mr. A. B. PETERS

Arthur Bowden Peters, who had held Life Fellowship in the Society since 1925, died on 24 Februar 1952 at the age of 62. He was for many years librarian and curator of Inverness Burgh and County Publ Library, and since 1950 had been head librarian for the whole of Inverness-shire. During the war o 1914-18 he received a captain's commission in the Royal Flying Corps and was severely wounded. Fou months after the outbreak of hostilities in 1939 he rejoined the service, retaining his post on the Roya Air Force staff until August 1945. Mr. Peters was a man of extensive scientific interests. Geology an botany, in addition to meteorology, had their claims on his time. He was something of an authorit on the semi-precious stones of Scotland. Latterly he set himself the task of investigating the suppose existence of a ' monster ' in Loch Ness. For this purpose he established on the neighbouring slopes a observation post from which he kept, over a period of two years, assiduous watch on the waters of th loch for any abnormal manifestations.

E.L.H.

Dr. E. H. HOWELL, M.Sc.

Edwin Henry Howell, a Fellow of the Society since 1948, died on 2 February 1952. He was born in Christchurch, New Zealand in 1915 and was educated at Christchurch Boys' High School and Canterbur University College, whence he graduated M.Sc. in 1937 with second-class honours in mathematics He was appointed to the staff of the Magnetic Observatory at Christchurch in 1935 and transferre to the Meteorological Office when it commenced operations at Wigram airfield in 1937. Subsequently he transferred to Auckland and during four years' war duty with the R.N.Z.A.F. saw oversea service in the Pacific theatre. In 1948 he was appointed officer-in-charge of the General Forecasting Office a Wellington, but shortly afterwards he was granted a rehabilitation scholarship and left to further hi studies at the Imperial College of Science and Technology, London. He obtained the degree of Ph.D and, on returning to New Zealand, had resumed his post only a short while before his untimely death

M.A.F.B.

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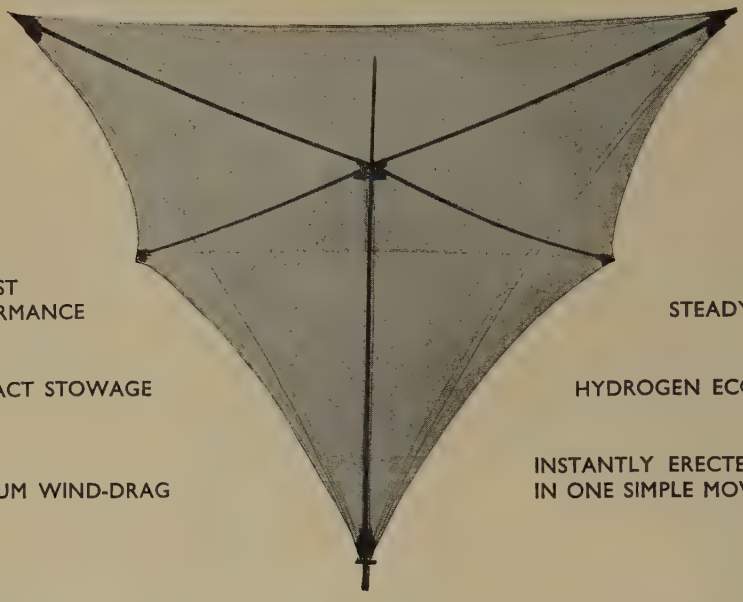

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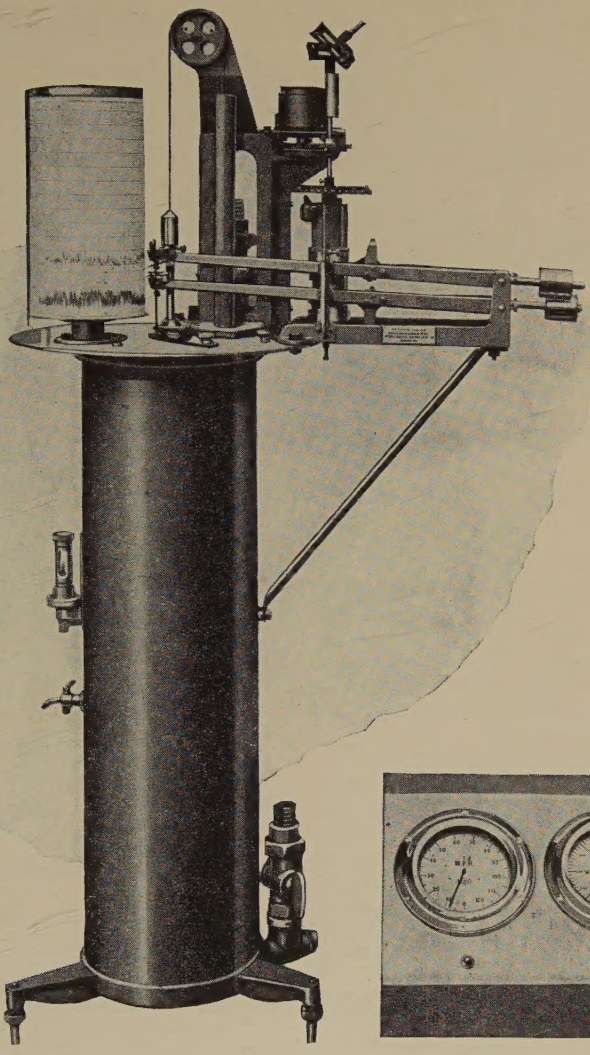

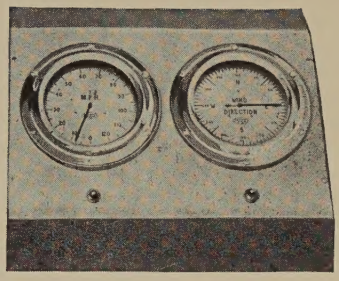

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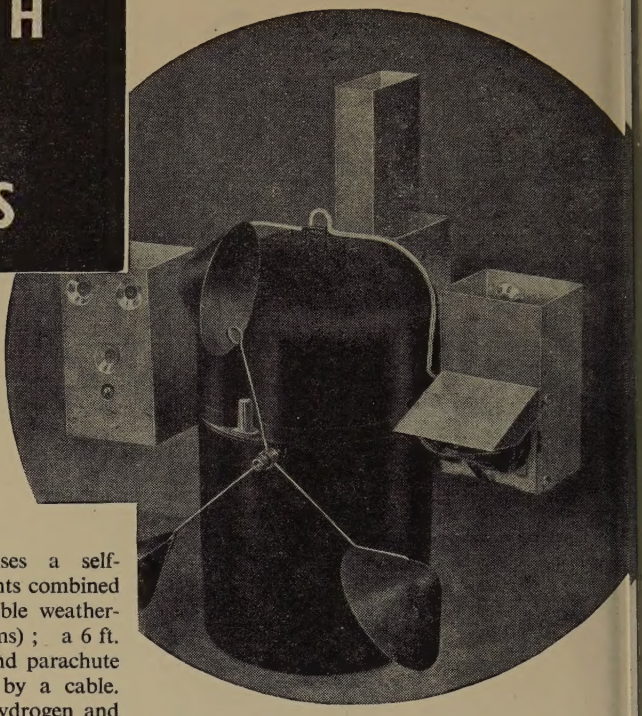

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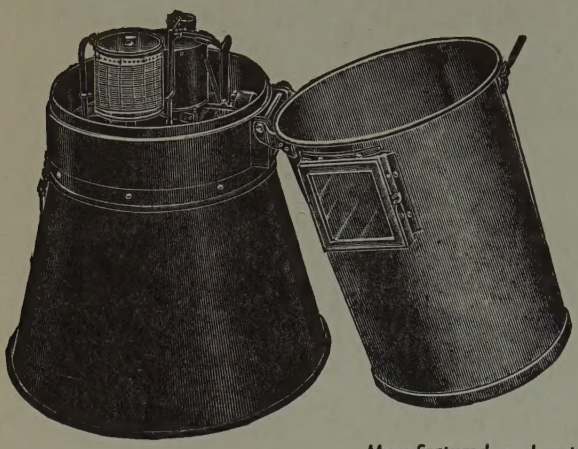

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